VOLUME 28 NUMBER 8

TJ1
.A72

DEDICATION OF BUILDING, APRIL 16-19, 1907

THE AMERICAN SOCIETY OF MECHANICAL ENGINEERS

PROCEEDINGS

APRIL, 1907

CONTENTS

SOCIETY AFFAIRS 1229
Dedication of Engineering Societies Building, April 16–19
Indianapolis Meeting, May 28–31
Announcements
OBITUARIES 1237
EMPLOYMENT BULLETIN 1240
CHANGES OF ADDRESS 1244
ACCESSIONS TO THE LIBRARY 1247
PAPERS FOR INDIANAPOLIS MEETING:
Preliminary Report of Committee on Refrigerating Machines 1249
Specific Heat of Superheated Steam, Mr. A. R. Dodge 1265
CONTRIBUTED DISCUSSION, DECEMBER MEETING:
Weights and Measures, H. R. Towne 1279
The Evolution of Gas Power, Frank Firmstone, F. E. Junge 1282
A Plan to Provide Skilled Workmen, John Knickerbocker, C. A. Williston, M. W. Alexander 1286
The Flow of Fluids in a Venturi Tube, C. A. Williston, E. P. Coleman 1294
A Mechanical Engineering Index, W. W. Bird, A. L. Smith 1296
Producer Gas Power Plant, J. R. Bibbins 1297
Ferroinclave Roof Construction, A. E. Brown 1301
Saw-Tooth Skylight in Factory Roof Construction, F. S. Hinds 1304
An Improved Transmission Dynamometer, W. F. Durand 1305
Test of a Rotary Pump, W. B. Gregory 1306
Steam Plant of the White Motor Car, R. C. Carpenter 1310
Boiler and Setting, A. Bement 1320
A High Duty Air Compressor, O. P. Hood 1325
Saw-Tooth Roofs for Factories, K. C. Richmond 1338
The Art of Cutting Metals, H. Le Chatelier, C. Codron, W. Lewis, R. A. Skeggs, J. T. Nicolson, F. W. Taylor 1339

INDIANAPOLIS MEETING, MAY 28-31, 1907

APRIL, 1907 VOL. 28. No. 8

THE AMERICAN SOCIETY OF MECHANICAL ENGINEERS

PROCEEDINGS

THE AMERICAN SOCIETY OF MECHANICAL ENGINEERS
2427-29 YORK ROAD, BALTIMORE, MD.

EDITORIAL ROOMS AND LIBRARY
29 WEST 39TH STREET, NEW YORK

Entered at the Post Office in Baltimore, Md., as second-class matter under the Act of July 16, 1894.

OFFICERS AND COUNCIL

PRESIDENT

FREDERICK R. HUTTON

VICE-PRESIDENTS

WALTER M. MCFARLAND	ROBERT C. MCKINNEY	P. W. GATES
EDWARD N. TRUMP	JOHN W. LIEB, JR.	ALEX. DOW

MANAGERS

GEO. M. BRILL	WALTER LAIDLAW	A. J. CALDWELL
FRED J. MILLER	FRANK G. TALLMAN	G. M. BASFORD
RICHARD H. RICE	FREDERICK M. PRESCOTT	A. L. RIKER

TREASURER

WILLIAM H. WILEY

SECRETARY

CALVIN W. RICE, 29 West 39th Street, New York

HONORARY MEMBERS

EDWIN REYNOLDS AMBROSE SWASEY JAMES M. DODGE
JOHN R. FREEMAN FREDERICK W. TAYLOR

STANDING COMMITTEES

FINANCE	*MEETINGS*	*MEMBERSHIP*
E. D. MEIER (*Chairman*)	A. E. FORSTALL	IRA H. WOOLSON (*Ch.*
ANSON W. BURCHARD	CHAS. WHITING BAKER	JESSE M. SMITH
ARTHUR M. WAITT	W. E. HALL	HENRY D. HIBBARD
EDWARD F. SCHNUCK	GEO. R. HENDERSON (*Ch.*)	C. R. RICHARDS
J. WALDO SMITH	L. R. POMEROY	F. H. STILLMAN

PUBLICATION	*LIBRARY*	*EXECUTIVE*
C. J. H. WOODBURY	FRED'K N. WHYTE	FRED'K R. HUTTON (*Ch.*)
FRED J. MILLER	GEO. F. SWAIN	FRED W. TAYLOR
WALTER B. SNOW	H. H. SUPLEE	JOHN W. LIEB, JR.
D. S. JACOBUS (*Ch.*)	AMBROSE SWASEY	FRED J. MILLER
H. F. J. PORTER	LEONARD WALDO	CALVIN W. RICE

The professional papers contained in Proceedings are published prior to the meetings at which they are to be presented, in order to afford members an opportunity to prepare any discussion which they may wish to present. They are issued to the members in confidence, and with the understanding that they are not to be published even in abstract, until after they have been presented at a meeting. All papers are subject to revision.

The Society as a body is not responsible for the statements of facts or opinions advanced in papers or discussions.

Proceedings is published twelve times a year, monthly except in July and August, semi-monthly in October and November. One copy of each issue without charge to members.

Price, one dollar per copy—fifty cents per copy to members. Yearly subscription, $7.50; to members, $5.00.

PROCEEDINGS

OF

THE AMERICAN SOCIETY OF MECHANICAL ENGINEERS

VOL. 28 APRIL, 1907 NUMBER 8

WEEK OF DEDICATION

TUESDAY AFTERNOON, APRIL SIXTEENTH

at three o'clock

DEDICATION EXERCISES

PRESIDING OFFICER, CHARLES WALLACE HUNT.

Music—Largo, Handel.

1 Presentation of the Gavel by Mrs. Andrew Carnegie.
2 Prayer, Dr. Edward Everett Hale.
3 Communications from the President of the United States, President of the Republic of Mexico, and the Governor General of Canada.
4 Historical Address by Mr. Charles F. Scott, the representative of the Conference and Building Committee.
5 Acceptance of the Building by Mr. E. E. Olcott, President of the United Engineering Society.
6 Address by Mr. Andrew Carnegie.

MUSIC

7 Oration, Dr. Arthur Twining Hadley, President of Yale University, "The Professional Ideals of the Twentieth Century."

Music—The Hallelujah Chorus.

(Admission to these exercises by invitation only)

TUESDAY EVENING, APRIL SIXTEENTH

RECEPTION

For Members, Ladies and Guests of the Founder Societies

from nine to ten-thirty o'clock

In the main Auditorium

Members will be received by the President of The American Society of Mechanical Engineers, Dr. Frederick Remson Hutton and Mrs. Hutton; by the President of The American Institute of Mining Engineers, Dr. John Hays Hammond and Mrs. Hammond; by the President of The American Institute of Electrical Engineers, Dr. Samuel Sheldon and Mrs. Sheldon; The President of the United Engineering Society, Mr. E. E. Olcott and Mrs. Olcott, and by the Chairman of the Reception Committee, Mr. John W. Lieb, Jr., and Mrs. Lieb.

At ten-thirty o'clock

In the Rooms of the Societies

The Officers and Councils of the Societies will receive the members and guests in the rooms of the respective Societies.

Supper will be served from ten until twelve o'clock and the entire building will be on view.

This reception is open to all members and ladies of the Founder Societies. Admission will be by ticket which may be secured from the Secretary.

Chairman of the Reception Committee, John W. Lieb, Jr.

WEDNESDAY AFTERNOON, APRIL SEVENTEENTH
at three o'clock

FOUNDERS' DAY

PRESIDING OFFICER, JOHN W. LIEB, JR.

1 Address by the President of The American Institute of Electrical Engineers, Dr. Samuel Sheldon.
2 Address by the President of The American Society of Mechanical Engineers, Dr. Frederick Remson Hutton.
3 Address by the President of the American Institute of Mining Engineers, Dr. John Hays Hammond.
4 Presentation of medals to the Secretaries by Dr. A. R. Ledoux, Past President United Engineering Society.
5 Presentation to the American Society of Mechanical Engineers of a portrait of George H. Corliss.
6 Presentation of the John Fritz medal, by Mr. Charles S. Scott
7 Oration—"Ethics of Secret Process in the Arts," by Dr. James Douglas, Past President of the American Institute of Mining Engineers.

Open to all members. No tickets required.

WEDNESDAY EVENING UNASSIGNED

PROFESSIONAL SESSIONS OF FOUNDER SOCIETIES

Every member of the Founder Societies is cordially invited to attend these sessions.

THURSDAY AFTERNOON, APRIL EIGHTEENTH
at two o'clock

SESSION OF THE AMERICAN INSTITUTE OF MINING ENGINEERS

A paper on "Mining Engineering in New York City," describing the excavation and tunnel work now being carried on by the Transportation Companies, will be read by Mr. H. T. Hildage.

THURSDAY EVENING, APRIL EIGHTEENTH
at eight-fifteen o'clock

SESSION OF THE AMERICAN SOCIETY OF MECHANICAL ENGINEERS

An address on "The Ordnance Department as an Engineering Organization" will be delivered by Brigadier General William Crozier, Chief of Ordnance, United States Army.

FRIDAY AFTERNOON, APRIL NINETEENTH
at two o'clock

SESSION OF THE AMERICAN INSTITUTE OF MINING ENGINEERS

Several professional papers on Metallurgy, Methods of Mining, Gas Engine Practice, the Treatment of Copper Ores, and various other subjects will be read and discussed at this and the Thursday afternoon session.

FRIDAY EVENING, APRIL NINETEENTH
at eight o'clock

SMOKER

An informal Smoker and Vaudeville for members of the Founder Societies in Madison Square Garden. Subscription, $2. Number of tickets limited. Applications from our members should be sent to the Secretary without delay.

This Smoker is to serve as a reunion and conclusion of the ceremonies of the week. The expense necessary for the entertainment has been incurred, and although the tickets are limited, it is important that all should be sold. Members are expected very generally to respond. Checks should accompany orders for tickets.

EXCURSIONS

Excursions will be arranged during the meeting to various engineering works in New York and vicinity.

MARCH MEETING

Mr. John W. Lieb, Jr., Vice-President of the Society, addressed a large audience in the main Auditorium on the evening of March 21. The lecture delivered on the occasion of the regular monthly meeting, was on Vesuvius and the Mechanic Arts of Pompeii, giving the history of the eruptions of Vesuvius and a description of the life and customs of Pompeii from the earliest record down to the present time.

The lecturer spoke of volcanic heat as a source of energy as yet practically without utilization, and as a field of investigation for the engineering profession, taking up also the character of volcanic activity, computing the enormous pressure necessary to sustain the columns of lava erupted from the craters—themselves in many cases a thousand feet deep—and to force them a thousand feet or more into the air.

He explained the accepted causes of volcanic activity, the theories of volcanic phenomena, and the products of eruption—giving a scientific analysis of their composition.

A brief historical sketch of the eruptions of Vesuvius, beginning in 63 A. D., followed, including the letters of Pliny the Younger to Tacitus, in which we have a graphic description by an eye-witness of the terrible catastrophe of 79.

The description and pictures of the city of Pompeii; the streets, the baths, the homes, the temples, and the amphitheatre formed a valuable part of the lecture. A picture was shown of an interesting relic called the Table of Standard Measures which is the first evidence of standardization of which we have any record. These standards were adopted during the rule of Augustus.

Many of the slides were specially imported. Some were taken by the author, others by Mr. Frank Perret of Brooklyn, who was in the Vesuvian Observatory during the eruption in 1906, and others were reproductions from drawings.

Moving pictures taken in April of last year, were shown personally by Mr. E. Burton Holmes, the eminent travelogue author and lecturer. Among them were pictures of Vesuvius in eruption, a panorama of lava, a flowing stream of molten lava, terrific outbursts of smoke, steam, sand and ashes, refugees leaving San Guiseppe and Ottaiano, and Vesuvius in fury as seen from Naples at sunrise.

We are greatly indebted to Mr. Lieb for providing a most interesting and profitable evening to members.

About one-third of the audience were ladies and the entire attendance was over one thousand, making it the largest meeting thus far of the Society in the new building.

DISCUSSION

Special sessions at the Indianapolis Meeting have been reserved for papers on Superheated Steam and the Automobile. Several research papers giving unusual data have been secured, and the opinions of the members are earnestly solicited for the discussion. The value of the Society to its membership depends largely upon the exchange of opinion and experience afforded by the meetings and recorded in the Society's publications. The members should respond generously with any contribution which they may be able to make to the sum total of knowledge upon the subject under discussion.

The Society may not know of individuals who have made researches along a certain line, and therefore be unable to make a personal request for the submission of a paper or discussion on a paper already presented, and we would urge that the members feel the responsibility to the profession to take the time to write up new data and results, or correct any apparently uncertain or unreliable information that may be presented.

Most successful men are very busy men, but by reason of their success they owe a larger debt to the profession in which they have attained eminence and should give in return the results of their wider experience and more correct methods.

It is the intention of the Society to publish as much discussion as possible before the meeting, and we shall be glad to receive promptly any contributions to papers appearing in this and subsequent numbers.

AMENDMENT TO BY-LAW 36

To conform with C59, the following amendment to B36 was voted by the Council to take effect immediately. The Secretary hereby notifies all members of this amendment.

"Ballots for an amendment to the Constitution shall be canvassed and announced in the same manner as the ballots for officers of the Society. The President shall appoint three Tellers to canvass any letter ballots which shall be ordered by the Council or by the Society and to certify the same to the President."

COMMITTEES

The retirement of Mr. Henry R. Towne from the Library Committee suggests some expression of appreciation of the interest which he has long felt in this department of the Society's work.

When the Mechanical Engineers' Library Association was incorporated in 1890, at the time of the purchase of the house in Thirty-first St., Mr. Towne was chosen as Chairman of the Board of Library Trustees, an office which he still holds, and which he has administered with much care and success. Some of the valuable books in the library have been presented by him, including sets of technical periodicals, and during the period in which he has served upon the Library Committee he has given valuable time and attention to its work. Although Mr. Towne now retires from active membership in the Library Committee there is no doubt that he still retains his interest in this important portion of the work of the Society, and it is to the interest of just such members that the development of the library has been due in the past, and that its still more rapid growth may be expected in the wider future which is opening before it.

The resignation of Mr. H. H. Suplee, Chairman, from the Publication Committee is received with regret by the Committee and by the Society. Mr. Suplee's close association with the Society during many years, his knowledge of precedent, his practical ideas, and willing services have been of great help in the administration of committee affairs.

Mr. Fred. J. Miller, member of the Council, has very kindly accepted the appointment on the Publication Committee made vacant by Mr. Suplee's resignation.

Dr. D. S. Jacobus has been chosen chairman.

THE LIBRARY

The libraries of the three Founder Societies are in the course of rearrangement and therefore are not entirely available until the books shall have been shelved and catalogued. Members are at liberty, however, to make use of the volumes in the usual way.

If any have doubted the policy of consolidation—they will find that the advantages to be gained from having at command the resources of the three libraries a strong argument in its favor.

PROF. HUTTON'S RESIGNATION FROM COLUMBIA.

"Dr. Frederick Remsen Hutton, for thirty years adjunct professor and professor of mechanical engineering at Columbia University and for six years dean of the faculty of applied science, will become professor emeritus on July 1 next. A suitable tablet commemorating Professor Hutton's services will be placed in the mechanical engineering laboratory."—Educational notes in "Science."

SEMI-CENTENNIAL MICHIGAN AGRICULTURAL COLLEGE

The fiftieth anniversary of the Michigan Agricultural College, Lansing, Mich., will be celebrated May 28 to 31. In response to the invitation from the college, the President has designated Prof. Paul Mellen Chamberlain, Los Angeles, Cal.; Prof. M. E. Cooley, University of Michigan, Ann Arbor, Mich.; Mr. Alex Dow, and Mr. F. E. Kirby, of Detroit, Mich., as Honorary Vice Presidents to represent the Society on this occasion.

THE FULTON CENTENNIAL

At a recent meeting, the Council voted that the vacancy caused by the death of Admiral Charles Harding Loring on the Committee on the Fulton Centennial be filled by the appointment each year of the President of the Society then in office.

RAILWAY TRANSPORTATION NOTICE

INSTRUCTIONS FOR PURCHASE OF RAILWAY TICKETS AND IN REGARD TO SPECIAL CARS, ETC., FOR THOSE ATTENDING THE SPRING MEETING AT INDIANAPOLIS, MAY 28–31.

A rate of a fare and a third for the round trip will be secured for members and guests attending the Spring Meeting. To obtain the rate:

a Ask for a certificate when you purchase your ticket to Indianapolis.

b Hand in this certificate with 25 cents when you register at Headquarters, or not later than May 31.

c Call later at Registration Desk and get your certificate after it has been signed by the Railway Agent.

Anyone buying a ticket to Indianapolis on May 24 to May 30 inclusive and obtaining a certificate from the ticket agent at the time of purchase can secure at Indianapolis, up to June 4, a non-transferable return ticket for a continuous trip over the same route for one-third the regular single fare.

The concession is subject to the condition that one hundred certificates should be turned in at the Registration Desk before the plan becomes operative. Everyone attending the Spring Meeting should obtain a certificate even if on account of returning by another route or for any other reason he does not intend to use it. Every certificate secured, irrespective of where obtained and whether used or not, applies upon the required minimum of 100. Therefore members who might otherwise not think it worth while on account of the shortness of the trip, or for any other reason to secure a certificate will see that their failure to do so may mean that no one attending the meeting will be able to secure the reduced rate. It costs absolutely nothing to secure a certificate.

To prevent all misunderstanding, call beforehand on the ticket agent from whom you mean to purchase your ticket, and find out whether his office has the appropriate blanks and instructions. In case of difficulty, write at once to the Society; and that you may have time to do this, make your preliminary inquiry in ample season.

Certificates can not be transferred to scalpers or brokers. The Society has to redeem at full fare any tickets transferred.

Arrangements for transportation, sleeping car and hotel accommodations, should be made personally by each person attending the meeting.

Members from the East wishing to return via Washington may do so without extra charge by securing tickets marked "via Washington." These tickets will be good for a trip either via Washington or by the more direct route. They are good for a ten day stop over in Washington.

SPECIAL TRAIN ARRANGEMENTS

FROM NEW YORK AND OTHER PENNSYLVANIA RAILROAD POINTS EAST

One or more private Pullman sleeping cars will be reserved on each of two trains from the East. These reservations will be made only in case the number of applications received for Pullman accommodations reach the minimum set by the company.

The first train selected is the "St. Louis Express" which will leave New York on Monday and arrive in Indianapolis during the afternoon on Tuesday. The first session of the meeting will be on Tuesday evening. The schedule for this train will be as follows:

Lv.	New York, Monday, May 27	6.25 p. m.
"	Philadelphia	8.58 p. m.
"	Pittsburg	5.37 p. m.
Ar.	Indianapolis, Tuesday, May 28	3.05 p. m.

The second train selected is the "Pennsylvania Limited," leaving New York on Tuesday and arriving in Indianapolis on Wednesday in time for the first professional session. New York members going by this train can have two hours in their offices on Tuesday.

Lv.	New York, Tuesday, May 28	10.55 a. m.
"	Philadelphia	11.10 p. m.
"	Pittsburg	8.55 p. m.
Ar.	Indianapolis, Wednesday	6.55 a. m.

FROM CHICAGO

The train out of Chicago upon which a special car will be run if the number of reservations warrants it, will reach Indianapolis in time for the opening session Tuesday evening. It is believed that our members and their guests will be glad to coöperate to secure this additional opportunity for social intercourse with old friends. The schedule will be:

Leave	Chicago, Tuesday, May 28	1.00 p. m.
Arrive	Indianapolis	6.20 p. m.

OBITUARY

GEORGE HENRY EVANS

George Henry Evans was born in Hull, Yorkshire, England, in July, 1866. He was educated in the ordinary home schools and private colleges and also studied engineering with his father, who was a civil engineer in Norwich, England. In 1890 he was sent out to New Zealand as general manager and resident engineer of the Round Hill Syndicate Mines, at Riverton, New Zealand. While there he had entire charge of all preparatory work in constructing canals, breakwater, and all mining operations. Later he was the manager and resident engineer of the Consolidated Gold Mines of California, Ltd., Mugalia Consolidated, Ltd., Golden Gate of California, Ltd., Morris Ravine Gold Mines, Ltd., Golden Feather, Ltd., the Development Syndicate, Ltd., and as consulting engineer with the Risdon Iron Works.

Mr. Evans' specialty was placer mining, and he was the inventor of the Evans hydraulic elevator, now generally used in the elevation of gravel.

He was a member of the North of England Society of Mining and Mechanical Engineers, the American Institute of Mining Engineers, The American Society of Mechanical Engineers, The Technical Society of the Pacific Coast, and Franklin Institute. He was also a member of the Bohemian Club of San Francisco. Mr. Evans died at Berkeley, California, February 4, 1907.

LOUIS SCHUTTE

Louis Schutte was born in 1842 in Hamburg, Germany. He attended the Polytechnic College, Hanover, graduating in 1862. He began his business career in Hamburg, later going to Berlin, and from there to London, progressing from position of draftsman to that of superintendent. Arriving in the United States in 1670, he established a company for the manufacture of injectors of the Koerting type and later added to this branch the manufacture and sale of

special and automatic valves, which are known in America under the name of Schutte valves.

At the time of his death Mr. Schutte was president of the Schutte and Koerting Company. He was a member of the Union Republican Club, the Union League Manufacturers' Club, the Engineers' Club, and became a member of the Society in 1887. He died in September, 1906.

JAMES BLAKE CAHOON

James Blake Cahoon was born at Lyndon, Vt., in 1856. He was educated in the Portland, Maine, Schools, and in the U. S. Naval Academy, graduating in 1879. He was also a graduate of the U. S. Torpedo School, Newport, R. I., and took special courses in electricity and the manufacture of torpedoes and high speed engines. On account of an injury received while making tests of arc lamps, he was placed on the retired list with the rank of Ensign.

Mr. Cahoon became associated with the Robinson-Foster Electric Motor Company, Boston, as chief electrical engineer, and in 1899 was managing director with the Boston Chemical Company. His next engagement was with the Thomson-Houston Electric Company as electrical engineer, later becoming manager of the special production department. In 1893 he went to Schenectady as head of the expert department of the General Electric Company, leaving there a year later to take up the management of the Elmira Railway and Electric Light Company. In 1899 he assumed the management of the Underground Electric Light Company at Syracuse, N. Y., and in 1901 associated himself with the banking house of Emerson McMillen and Company, of New York, and later with Furson, Leach and Company, leaving this business to establish himself as consulting engineer. In 1905 he was connected with the Eldenbel Construction Company, and was made vice-president and chief engineer. He was past president of the National Electric Light Association, and was a member of several engineering societies. Mr. Cahoon became a member of this Society in 1892. He died February 17, 1907.

ARTHUR VAUGHAN ABBOTT

Arthur Vaughan Abbott, was born in Brooklyn, N. Y., in 1854. After graduating from the Polytechnic Institute of Brooklyn in 1875, he served in the Department of Parks of New York, and in the engineers' department of the East River Bridge, superintending cable

construction, and in charge of material inspection. Among his noteworthy inventions and patents were the cable wire splice, wire cutting machinery, and testing machines for steel and cement.

Mr. Abbott was associated with E. T. Fairbanks & Company four years; the Boston Heating Company as chief engineer three years; with the Daft Electric Light Company as mechanical engineer, where he was engaged in work on the early electric roads. He was also consulting engineer for the Ogden City Water Works, the Bear Canal, and the Waukesha Water Pipe Line.

In 1892 Mr. Abbott took up telephone construction. He was the author of "A Treatise upon Fuel," "Testing Machines," "Telephony," and numerous magazine articles and professional papers.

Mr. Abbott was a member of the American Institute of Electrical Engineers, The American Society of Civil Engineers, and became a member of this Society in 1901. He died in St. Luke's Hospital, New York, December 1, 1906.

EMPLOYMENT BULLETIN

The Society has always considered it a special obligation and pleasant duty to be the medium of securing better positions for its members. The Secretary gives this his personal attention and is most anxious to receive requests both as to positions and as to men available. Notices are not repeated except upon special request. Copy for notices in this Bulletin should be received before the 20th of the month. The list of men available is made up entirely of members of the Society and these are on file with the names of other good men not members of the Society, capable of filling responsible positions, information about whom will be sent upon application.

POSITIONS AVAILABLE

027 Suitable man for business manager. A man of engineering training with commercial experience. Location, New York State.

028 Assistant to head draftsman. Must have had extended experience in machine tool designing and shop experience; a man with technical education preferred. Location, 12 miles from Philadelphia.

029 Technical graduate with practical experience sufficient to qualify for a position as fire department engineer in connection with the work of Committee of the National Board of Fire Underwriters. Work would be the testing of fire engines in various cities and the gathering of data on the fire department and fire alarm system in a city. Good ability in English composition is essential.

030 Salesman wanted for the sale of condensers by a Philadelphia concern.

031 Mechanical engineer: A Portland Cement Company, located in Lehigh Valley district, which expects to build a new plant in about two years and use the present superintendent to build and operate new plant, wants engineer with experience (knowledge of cement machinery not necessary) to join present organization with view of becoming superintendent of present plant when new plant is started, provided he makes good. Give details of experience and state salary expected.

032 Experienced mechanical draftsman wanted by manufacturing concern near Boston, for designing jigs, fixtures, and special machinery. Permanent position for the right man. Draftsman with practical shop experience preferred. Address with full particulars regarding age, education, experience, and initial salary expected.

033 Superintendent for pattern shop, machine shop and foundry; man of experience in modern shops and shop methods whose duties would be the execution of orders after they were placed in the shops. A man who can secure the greatest possible output without sacrificing the quality of the product; tactful in encouraging intelligent effort in subordinates. Corliss steam engines, gas engines, producers and transmission machinery. Location, Minnesota.

034 Shop doing general work, repairs, etc., with the view of catering to the trade for fine work, and ultimately to developing some special line of manufacture, want thoroughly competent and practical man who understands employment and management of men, capable of properly estimating cost and turning out work in a satisfactory manner; to take full charge of shop as general manager or superintendent. Location, Washington.

035 Steam engineer to superintend construction of power stations and be responsible for maintenance from steam end. Competent engineer and business man. Salary, $3000. Location, New York.

036 Large casualty corporation invites applications from engineers of sound mechanical judgment who are experienced in the design and construction of steam boilers and elevators, and familiar with general factory equipment and operation. Salary dependent upon ability. Location, New York.

037 Electrical engineer for power house work. Also draftsman to work up designs; salary, about $35.
Communicate with Mr. Frank S. Howell, engineer in charge, Ellis Island, New York.

MEN AVAILABLE

In addition to the list of men available, the Society is pleased to announce that one of the colleges has volunteered to undertake to supply both undergraduate and graduate students for part time, summer, and permanent positions. The Secretary will be pleased to be the medium for correspondence.

49 Associate member, 35 years of age, practical and technical. Extended experience in design and construction of machinery and mechanical appliances, mill, factory construction and equipment. Thoroughly up-to-date designer of special automatic machinery and machine tools, jigs, fixtures, punches, and dies, wood and metal

patterns, limit gages for the production of fine and medium heavy interchangeable work. Excellent executive ability and can produce results. Location, New York and vicinity preferred.

50 Member of Society, twenty years' experience as salesman, office manager, auditor and sales manager—could qualify as manager of branch office or in sales department. Especially familiar with steam engineering.

51 Mechanical and civil engineer, technical graduate, 14 years' experience in mechanical, structural, and electrical engineering. Held positions as engineer and chief draftsman. Qualified for a responsible executive position, prefer one requiring ability in both mechanical and structural engineering.

52 General manager or manager of works for large manufacturing concern employing 2500 men and upward. Broad experience in organization, engineering, and production.

53 Mechanical engineer engaged in teaching in a technical college of the first rank, desires a position in practice during summer vacation of 1907. Good experience in executive work, including the handling of men; engineering calculation and design; testing and experimental work; inspection of machinery and engineers' tools, stores, and supplies. Power plant work a specialty.

54 Mechanical and electrical engineer; age 30; technical graduate 12 years' practical experience, desires change for position as assistant to chief engineer, superintendent, manager or similar position. Location, preferably Philadelphia or vicinity.

55 Technical graduate, age 30, four years' experience in fire insurance business, inspecting, adjusting, and assistant secretary. Previous experience, 2½ years as draftsman with the department of construction and repair, Navy Department. Experimental work, designing, and estimating. Desires position with manufacturing or contracting concern where executive ability is needed, as well as engineering experience.

56 Mechanical engineer of long training and practical experience in designing and building machinery desires position as instructor in mechanical engineering, machine design, machine shop work, etc., in a technical or trade school where executive ability would also be desirable.

57 An author of well known mechanical books, a mechanical engineer and practical mechanic of much experience, seeks position with technical or mechanical journal or publishing house where practical knowledge and literary ability would be considered.

58 Mechanical and electrical engineer desires change. Technical graduate, eight years' experience in machine shop, two years in design and drawing room, and two years as manager and chief engineer on engineering, contracting and construction work, employing over two hundred men, several years' teaching experience in mechanical and electrical engineering subjects, the past two as professor of electrical engineering, in charge of the department, in a state school of technology. Would accept satisfactory position in either teaching or commercial lines.

59 Technical graduate, age 38, practical mechanic. Three years general drafting work. Five years general foreman of machine shop, forge and power plant employing 300 men. Experienced in jig and tool work for rapid production of interchangeable parts. Eight years in charge of designing and developing new work for a large concern. Personally developed some twenty patented constructions. Posted on properties of special steels and metal alloys, modern gas engines, gasolene engine and automobile construction. Would like to connect with some concern that offers an opportunity of working into and purchasing part interest in the business.

60 Member, 32 years old, desires position with small institution. Successful in factory management, experienced in handling all classes of mechanics and laborers. Wide experience in engine and boiler practice, electrical machinery, and factory equipment.

61 Mechanical engineer, age 49, expert in designing and testing hydraulic machinery, air compressors, Corliss engines.

62 Electrical and mechanical engineer—technical graduate, 12 years' experience in shops, drafting, designing and estimating, desires a change. Can handle men and does not mind hustling.

63 Graduate of Sheffield Scientific School—Yale, at present head draftsman with responsible concern desires a change. Extended experience in machine design and acquainted with shop practice. Can make good.

64 Position desired in line of refrigerating machinery or gas engines work; good experience Recently designed a complete automatic system of refrigeration.

65 Draftsman on gas engine work.

66 Position desired with first class concern in selling organization; experienced and fully qualified to hold position of responsibility. Technical and practical knowledge and broad experience in commercial field. Present position one of prominence with large manufacturing company. References as to ability, character, etc.

CHANGES OF ADDRESS

AIKEN, Charles Wilson (1903), Chief Engr., The Proctor & Gamble Co., Mariners Harbor, and *for mail*, 101 Castleton Ave., West New Brighton, Staten Island, N

AUCHINCLOSS, Wm. S. (1881), Civil Engr., Atlantic Highlands, N. J., and *for mail*, 32 East 68th Street, New York, N. Y.

AYRES, Gustav (1906), Patent Expert and Cons. Engr., 1006 F Street N. W., Washington, D. C.

BAILEY, Erwin G. (Junior, 1903), 93 Broad St., Boston, Mass.

BEAN, I. McC. (Junior, 1903), Manager, Sidney A. Bean & Brother, 1773 Broadway, New York, N. Y.

BROWN, James Monroe (1900; 1906), The Casey Hedges Co., Chattanooga, Tenn.

BUCKLER, Albert (Junior, 1905), The Westinghouse Machine Co., 171 La Salle St., Chicago, Ill.

CAVAGAN, Edward Francis (Junior, 1906), 626 West 5th Street, South Boston, Mass.

CHURCH, Charles Thos. (Junior, 1895), 63 Wall St., New York, and *for mail*, Davenport Neck, New Rochelle, N. Y.

COX, Sterling B. (Junior, 1901), care of Robt. Winthrop & Co., 40 Wall St., and *for mail* P. O. Box 17, New York, N. Y.

DAM, William Valdemar (1906), Asst. Genl. Mgr. and Engr., Thompson & Co., and *for mail*, Berkeley St., Castlemaine, Victoria, Australia.

DASHIELL, Benj. J. (Junior,1890), Cons. Engr., 823 Equitable Bldg., Baltimore, and Athol Terrace, Athol, Baltimore County, Md.

DONOVAN, Wm. F. (1883), Consolidated Water Improvement Co., 2038 Land Title Building, Philadelphia, Pa.

DOUGLAS, Courtney Carlos (Junior, 1904), Constr. Dept., Genl. Elec. Co., Schenectady, N. Y.

EASTMENT, Wm. H. (Junior, 1896), Engr., B. Altman & Co., 5th Ave and 34th St., New York, and Glen Cove, L. I., N. Y.

FOLLER, Charles S. (1906), M. E. and Asst. Treas., Union Spring & Mfg. Co., 616 Farmers Bank, Pittsburg, Pa.

FORSYTH, William (1883), Mech. Engr., 160 Harrison St., and Windermere Hotel, Chicago, Ill.

FRANCIS, Charles Albert (Associate, 1906), Instr. and Master Mechanic, The Higher Technological School, Tokio, Japan.

GAINES, Frederic F. (1901), Supt. Motive Power, Central of Ga. R. R. Savannah, Ga.

GALLOUPE, Francis E. (1880), Mech. Engr., Room 733 Old South Building, and 192 Marlborough St., Boston, Mass.

GAZZAM, Joseph P., (1902) Life Member; Nevada Consolidated Copper Co., Ely, White Pine Co., Nev.

GILLETTE, James Walter (Associate, 1904), Mgr., Ft. Smith Light & Traction Co., Fort Smith, Ark.

GLASGOW, Carr Lane (Junior, 1904). Present address unknown.
GUMP, Walter B. (Junior, 1902), 2510 Juliet Ave., Los Angeles, Cal.
HARTILL-LAW, A. (1906), Phœnix Fdy. & Eng. Works, Spence St., Cairns, North Queensland, Australia.
HEAP, Douglas T. (1906), Head Chief Mech. Engrs. Dept., Royal Arsenal, Woolwich, London, S. E., England.
HIGGINS, Albert Willis (Associate, 1906), 1105 Times Bldg., St. Louis, Mo.
HOLMES, George Lewis (1899; 1906), Cons. Engr., 140 Santa Clara Ave., and *for mail*, 950 Fifth Ave., Oakland, Cal.
HOLT, Charles Parker (Associate, 1906), 914 Falsom St., San Francisco, Cal.
HOLZ, Fred. H. Jr. (Associate, 1903), 817 N. Broad St., Philadelphia, Pa
HURD, Hobert J. (1894; 1897), Supt., Robin Hood Powder Co., Swanton, Vt.
JACOBS, Henry William (1905), Asst. Supt. M. P., Atchison, Topeka & Santa Fe Ry. System, Topeka, Kan.
JETT, Carter C. (Junior, 1902), General Delivery, Youngstown, O.
JONES, Benjamin N. (1904), Mfg. and Constr. Dept., Otis Elevator Co., 17 Battery Pl., New York, N. Y., and *for mail*, 308 Essex Ave., Orange, N. J.
KING, W. Grant (1894; 1904), Pres., W. G. King Co., 254 Court St., and *for mail*, 141 Park St., Buffalo, N. Y.
KOTHNY, Gottdank Lebrecht (Junior 1905), Ch. Engr. Condenser Dept., Skodawerke, A. G. Pilsen, Bohemia, and *for mail*, 80 Parkstrasse, Troppau, Austria.
KRUESI, August H. (1901; Associate, 1904), Designing Engr., General Electric Co., and *for mail*, 225 Union St., Schenectady, N. Y.
LANE, Francis W. (Associate, 1900), "The Railway Age," 150 Nassau St., New York, N. Y.
LAWRENCE, J. P. Stuart (1885), Commodore, U. S. N., The Art Club, Philadelphia, Pa.
LEITCH, Meredith (1898), Mech. Engr., Waverly Park, Newark, N. J.
LIBBY, Sam. H. (1897; 1900), Engr., Hoist. Dept., Sprague Electric Co., Bloomfield, and *for mail*, 23 Whittlesey Ave., East Orange, N. J.
MILLER, J. S. K. (Junior, 1900), Mech. Engr., Estimating Engr., Bucyrus Co., and *for mail*, Box 45, S. Milwaukee, Wis.
MOREHOUSE, Wm. S. (1896; 1989), Works Mgr., The Green Fuel Economizer Co., and *for mail* Matteawan, N. Y.
MOWERY, John Norris (1906), Mech. Engr., Lehigh Valley R. R., and *for mail*, 738 Seneca St., S. Bethlehem, Pa.
NICKEL, Franz F. (1899), Cons. Engr., 27 Winans St., East Orange, N. J.
NICKERSON, Alvano T. (1901), Mech Engr., care of Stone & Webster Engineering Corporation, 15 Exchange St., Boston, Mass.
PELL, David W. (Associate, 1903), 50 Magnolia St., Hartford, Conn.
POST, George Adams, Jr. (Junior, 1906), Engr. Representative, Standard Coupler Co., 160 Broadway, and *for mail*, 152 West 105th Street, New York, N. Y.
PRITCHARD, W. S. (1903), Genl. Mgr., Great Northern Portland Cement Co., Marlborough, Mich.
REID, John Simpson (1898), Yale & Towne Mfg. Co., 15 Broad St., Stamford, Conn., and 116 Stewart Ave., Ithaca, N. Y.
RENOLD, Charles Garonne (Junior, 1906), care of Hans Renold, Ltd., Progress Works, Manchester, England.
RIGGS, John D. (Junior, 1882), 606 Oak St., Dayton, Ohio.

SCHWARTZ, Carl (1906), Engr. of Power Station, N. Y. C. & H. R. R. Co., Grand Central Station, Room 1232, and 343 St. Nicholas Ave., New York, N. Y.

SLADE, Arthur J. (1904), Cons. Engr., Room 708, Times Building, 42d St. and Broadway, and 47 E. 58th St., New York, N. Y.

SLAWSON, Harry H. (Junior, 1904), 345 Eastern Parkway, Brooklyn, N. Y.

SMITH, Ernest L. (Junior, 1901), Mech. Engr., care of Shaw Electric Crane Co., Muskegon, Mich.

SMITH, Roy Brooke (Junior, 1905), 60 Jefferson Ave., Columbus, Ohio.

STILLMAN, Howard (1895), 1180 Flad Building, San Francisco, and 2434 Piedmont Ave., Berkeley, Cal.

STROM, Carl A. (1905), care of U. S. Metallic Packing Co., Great Northern Bldg., Chicago, Ill.

SULLIVAN, Lucien N. (1903), Engr. Mines Division, Eng'g Dept., Compania de Real del Monte Y Pachuca, Apartado 39, and *for mail*, 2a Calle de Hidalgo Num. 19, Pachuca, Hdgo. Mexico.

SWENSON, Bernard Victor (1903), Secy. and Treas., American Street and Interurban Ry. Assn., 29 West 39th Street, New York, N. Y.

TABOR, Harris (1880), Cons. Engr., Tabor Mfg. Co., 18th and Hamilton Sts., and *for mail*, Hamilton Court, 39th and Chestnut Sts., Philadelphia, Pa.

TRACY, Theron H. (1902), care of Traylor Engineering Company, 118 Liberty St., New York, N. Y.

TREGELLES, Henry (1888), care of Norton, Megaw & Co., Rio de Janeiro, Brazil, South America.

TRUELL, Karl Otto (Associate, 1906), Asst. Engr., W. S. Baiston & Co., 56 Pine St., and *for mail*, 525 W. 135th Street, New York, N. Y.

VEITCH, Thomas (Junior, 1901), Cons. Automobile Engr., 103 East 125th St., New York, N. Y.

WHEELER, Wm. Trimble (1905), 107 Liberty St., New York, N. Y.

WHITE, Edward F. (1891), 125 Burke St., Easton, Pa.

WILLIAMS, Ernest D. (Junior, 1905), Turbine Eng. Dept., General Electric Co., and *for mail*, 6 Brandywine Ave., Schenectady, N. Y.

ACCESSIONS TO THE LIBRARY

Donations[1]

THE STEAM ENGINE AND OTHER HEAT MOTORS. By W. H. P. Creighton, U. S. N. (Retired). *John Wiley & Sons, New York. Chapman & Hall, Limited, London, 1907.* 8vo, xii, 499 pages, 198 figures. Cloth, $5 (21/net).

Contents, by chapter headings: Review of Elementary Principles and General View of Steam Engine Plant; Steam Engine Indicator and its Calibration; Curves and Work of Expansion; Zeuner and Bilgram Valve Diagrams and Design of Plain Slide Valves; Measuring the Effects of Heat; Measuring the Effects of Heat on Water and Steam; Measurement of Heat Losses; Entropy; Condensers and Air Pumps; Small Auxiliaries; Multiple Expansion Engines; Revolution Control; Speed Variation Control; Steam Engine Tests; Superheated Steam and Steam Turbines; Gas Engines and Gas Producers; Boiling in a Vacuum; Refrigeration.

Appendix; Table 1, Properties of Familiar Substances; Table 2, Hyperbolic or Naperian Logarithms; Table 3, Heating Values of Various Substances; Table 4, Oxygen and Air Required Theoretically for the Combustion of Various Substances.

Exchanges and Purchases

PROCEEDINGS OF THE 16TH ANNUAL CONVENTION OF THE ASSOCIATION OF RAILWAY SUPERINTENDENTS OF BRIDGES AND BUILDINGS. *Concord, N. H., 1906.*

PROCEEDINGS OF THE ENGINEERS' SOCIETY OF WESTERN PENNSYLVANIA. *Vol. 23, No. 1, February, 1907.*

PROCEEDINGS OF THE AMERICAN INSTITUTE OF ELECTRICAL ENGINEERS. *Vol. 26, No. 2, February, 1907.*

PROCEEDINGS OF THE ENGINEERS' CLUB OF PHILADELPHIA. *Vol. 24. No. 1, January, 1907.*

PROCEEDINGS OF THE INSTITUTION OF MECHANICAL ENGINEERS. *No. 3, 1906. London.*

MÉMOIRES ET COMPTES RENDUS DES TRAVAUX DE LA SOCIÉTÉ DES INGÉNIEURS CIVILS DE FRANCE. *Bulletin de Décembre, 1906.*

SMITHSONIAN INSTITUTE, ANNUAL REPORT FOR 1905. *Washington, 1906.*

NEW YORK STATE MUSEUM 57TH ANNUAL REPORT, APPENDIX 8 AND GENERAL INDEX. *Vols. 3 and 4, Albany, 1905.*

BUREAU OF STEAM ENGINEERING, ANNUAL REPORT, NAVY DEPARTMENT. *Washington, 1906.*

SACHSISCHER DAMPFKESSEL—REVISIONS VEREIN. INGENIEUR-BERICHT. *Chemnitz, 1906.*

[1] Books in this list have been donated by the publishers.

BULLETIN DE LA SOCIÉTÉ INDUSTRIELLE DE MULHOUSE. *November, 1906.*

BULLETIN OF THE BUREAU OF LABOR, No. 68. DEPARTMENT OF COMMERCE AND LABOR. *Washington, January, 1907.*

QUARTERLY BULLETIN OF THE COMMITTEE ON SPECIAL HAZARDS AND FIRE RECORD OF THE NATIONAL FIRE PROTECTION ASSOCIATION. *No. 16. January, 1907.*

SECOND REPORT OF THE LOCOMOTIVE COMMITTEE ON STANDARD LOCOMOTIVES FOR INDIAN RAILWAYS. The Engineering Standards Committee. *London, February, 1907.*

DONATIONS FROM MEMBERS

ENGINEERING EXPERIMENT STATION OF ILLINOIS. *Vol. 1, Bulletins 1-8, Circulars 1-2, Urbana, Ill., 1906.*

SEWAGE DISPOSAL OF PATERSON, NEW JERSEY, 1906.

STEAM TURBINES. By Lester G. French.

PRELIMINARY REPORT

Preliminary Report of the Committee on a Code of Rules for Conducting Tests of Refrigerating Machines

[Subject to Revision[1]]

To the American Society of Mechanical Engineers,

Gentlemen:

The Committee, appointed by the Society in December, 1903, to suggest a Standard Tonnage Basis for Refrigeration, and reappointed in 1904 to recommend a method for measuring the quantity of refrigerating fluid evaporated in the refrigerator, and to prepare a code of rules for conducting tests of refrigerating machines, begs leave to report as follows:

1 Your Committee rendered a report on the first part of their work, namely, "A Standard Tonnage Basis for Refrigeration" which was accepted at the New York meeting in 1904. In this report standard units to measure the cooling effect or the refrigeration produced were recommended, together with a standard set of conditions under which a refrigerating machine, no matter what its type, should be run in determining what was designated as its commercial tonnage capacity.

2 The unit adopted to measure the cooling effect or the refrigeration is the heat required to melt one pound of ice, which is 144 British thermal units, and by dividing the refrigeration measured in British thermal units by 144, the ice melting capacity in pounds is obtained. The unit for a ton of 2000 pounds of ice melting capacity is therefore 288,000 British thermal units. The tonnage capacity is the refrigerating capacity expressed in tons of ice melting capacity in 24 hours, and is equivalent to the abstraction of 288,000 British thermal units in 24 hours, or to 12,000 British thermal units per hour, or 200 British thermal units per minute.

To be presented at the Indianapolis (May, 1907) meeting of The American Society of Mechanical Engineers and to form part of Volume 28 of the Transactions.

[1] It is expected that many corrections or alterations may be necessary in this report and it is submitted with a view of bringing out discussion.

3 The unit recommended for measuring what is termed the commercial tonnage capacity is based upon the actual weight of refrigerating fluid circulated between the condenser and the refrigerator, and actually evaporated in the refrigerator. In other words, for an ammonia machine the commercial tonnage capacity is based on the amount of anhydrous ammonia evaporated in the brine cooler or in any other type of ammonia evaporator or refrigerator. It was further stated in our first report that the actual refrigerating capacity of a machine may be determined from the quantity and range of temperature of the brine, water, or other secondary refrigerating liquid circulated as a refrigerant, and that the actual refrigerating capacity under the standard set of conditions should correspond closely to the commercial tonnage capacity.

4 The standard set of conditions recommended are those which often exist in ice making, namely that the temperature of the saturated vapor at the point of liquefaction in the condenser is 90° F. and the temperature of the evaporation of the liquid in the refrigerator 0° F. This corresponds for ammonia to a condenser pressure of about 168 pounds per square inch above the atmosphere (commonly called gage condenser pressure), and to a pressure of about 15 pounds per square inch above the atmosphere in the refrigerator (commonly called gage back pressure).

5 It was appreciated by your Committee, at the time of rendering their first report, that it would sometimes be practically impossible to obtain the commercial tonnage capacity which they recommended. For example, in a wet compression machine, unless special means are employed to obtain the amount of liquid anhydrous ammonia leaving the cooling coils along with the ammonia gas, it will be impossible to say exactly how much liquid ammonia is evaporated in the cooling coils. Again, in an absorption machine the effect of any water which may be found to enter the refrigerator along with the ammonia will be difficult to estimate. In certain cases it may also be practically impossible to determine the actual refrigerating capacity, as for example, where cooling is done by expanding the ammonia directly in coils which come in contact with the air of the rooms of a cold storage warehouse and where substances are refrigerated of unknown specific heats.

6 Your Committee appreciates the fact that where a machine is put in under a contract to do some specific work that the ability of the machine to do this specific work is of the first importance, and that in formulating any rules this must be kept in mind. That is, the subject must be viewed from the standpoint of the purchaser and

manufacturer as well as from that of the engineer. In preparing the directions for conducting the tests which follow, your Committee has proceeded with this in view, and has endeavored to formulate a set of rules which will appeal to all.

7 No attempt was made in our first report to recommend a commercial tonnage capacity for other machines than those which perform refrigeration by evaporating a refrigerating fluid. The general rules given, however, apply to all machines. In the case of air machines, the actual tonnage capacity for a specified set of conditions is obtained by basing the refrigeration on the amount of air cooled and the amount which it is lowered in temperature.

8 In the Code of Rules which follow, the primary refrigerating fluid is considered to be ammonia, as this is most commonly used, but generally speaking, the rules will apply no matter what the refrigerating fluid may be.

9 In speaking of the refrigerating capacity of a machine only that part of the plant which has the ammonia or other primary refrigerating fluid in circuit is considered. The capacity of that part of the plant where the cold brine or other secondary fluid is made use of for refrigeration is not considered. In a brine circulating system where brine coils are made use of to produce the refrigeration the capacity of these coils is not therefore taken into account. A test made with a brine heater gives correctly the capacities herein specified.

Respectfully submitted,

PHILIP DEC. BALL,
D. S. JACOBUS,
E. F. MILLER,
A. P. TRAUTWEIN,
G. T. VORHEES,
Committee.

RULES FOR CONDUCTING TESTS OF REFRIGERATING MACHINES

CODE OF 1907

10 There are a number of similar features to be considered in all classes of tests. For example, the engineer conducting a test should first satisfy himself regarding the object of the test. He should examine the general condition of the plant, and make sure that it is in as good condition as circumstances will allow. This part of general testing work has been described so thoroughly by various committees on tests having already reported to the Society, that it will not be gone over again here.

CALIBRATION OF THERMOMETERS

11 All the thermometers used should be carefully calibrated before employing them in a test. The 32° point may be determined by noting their readings when surrounded by melting ice, and other points by comparing with a standard thermometer which should also be calibrated at its ice point in order to make sure that it is correct. In a standard thermometer the corrections are constant throughout the entire range, or if variable the amount of variation is known. It is necessary to compare the thermometers with the standard at various points even if they are found to be correct at the ice point. Metallic case thermometers should never be used where accurate results are desired as the metallic tops conduct heat to and from the bulb and introduce an error. Thermometers having the graduations marked directly on the glass stems should be used, and these should be placed in wells containing brine or mercury, the wells to extend for at least two inches into the space where the fluid circulates. The mercury in the stem of the thermometer should stand a little higher than the top of the well in order that the readings may be obtained without moving the thermometer. It is advisable, however, not to have the mercury columns exposed to too great a height above the well; for if this is done there may be an appreciable error on account of the difference in temperature of the column so exposed and that of the well. Where the range of temperature through which the refrigerating fluid is cooled is measured in order to determine the capacity of the machine, it is often necessary to measure this range with the highest degree of refinement. For example, if a refrigerating machine cools brine through a range of 5°, one-tenth of a degree will be equivalent to 2 per cent of the range of temperature, and it is therefore essential that the range should be determined with as great accuracy as possible. In general, it is well to interchange the thermometers which are used for measuring the temperatures of the inlet and outlet brine several times during a test, making note of such changes on the record of the test

CALIBRATION OF WATER AND BRINE METERS

12 Where meters are used for determining the amount of refrigerating fluid which is circulated they should be carefully calibrated, both before and after a test, and in some cases, where long tests are made, they should also be calibrated during the test.

13 For the proper method of connecting up and calibrating meters, see the report of the Committee on Standard Engine Tests, Transactions A. S. M. E., Volume 24, p. 723. In calibrating a meter

the measurements should be made with the meter in the position in which it is installed in the test. This is especially necessary where the liquid which is measured is circulated by means of a pump which produces pulsations in the pressure, because the pulsations, as well as the total pressure, must be the same in calibrating the meter as exist in the actual test. In calibrating a meter with either water or brine the temperature of the fluid should be about the same as exists in the test.

DURATION OF TEST

14 The duration of a test depends upon its character. If a test is made of an ice making plant, and it is desired to obtain the actual amount of ice made per day or the pounds of ice made per pound of steam consumed, it may be necessary to make tests of say a week or more in duration in order to eliminate as far as possible any error in estimating the amount of ice and cold stored in the freezing tank, which should be made as nearly as possible the same at the end as at the beginning of the test.

15 Where the refrigerating capacity is measured, the conditions should be made as nearly the same as possible at the beginning and the ending of a test. It is impossible, however, in many cases to make sure that the conditions are the same and this is especially so when the anhydrous ammonia pockets in some parts of the machine for a time and then reappears at the ammonia receiver. By making the test of a long enough duration, any error involved through such irregularities will be practically eliminated and in most cases all tests should be of at least eight hours' duration.

16 It is essential that the average temperature of that part of the brine between the points where its temperature is measured and where it is cooled by the evaporation of the ammonia, as well as the quantity of this part of the brine, be the same at the end as at the start of the test. If there is much difference in temperature or quantity a correction should be applied.

CONDITIONS EXISTING IN TESTS

17 Where a machine is guaranteed to develop a certain capacity with a certain quantity of condensing water at a certain temperature, it is often necessary to heat the condensing water to the temperature specified in the contract. This may readily be done by circulating the water through a heater in which steam is admitted to bring the condensing water to a desired temperature.

18 All conditions specified in a contract should be followed as closely as possible in making a test.

MEASUREMENT OF THE AMOUNT OF ANHYDROUS AMMONIA CIRCULATED AND EVAPORATED IN THE COOLING COILS

19 The anhydrous ammonia must necessarily be measured under pressure. The best method is actually to weigh it, employing two tanks having flexible metallic pipe connections for the purpose.

20 The arrangement of the two ammonia cylinders for measuring the anhydrous ammonia is shown in Fig. 1. The ammonia receiver installed with the machine is marked *A*, and one of the two tanks for weighing the anhydrous ammonia *B* and the other *K*. In using the tanks for weighing anhydrous ammonia the valve *D* is closed. In filling the tank *B*, the valves *E* and *F* are opened, and the valve *G* is closed. After the tank *B* is filled, the valve *E* is closed and the weight

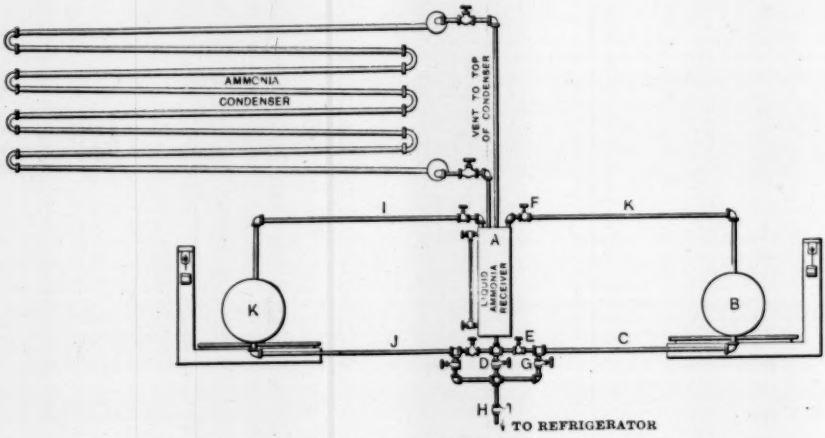


FIG. 1 SHOWING ARRANGEMENT OF TWO AMMONIA CYLINDERS FOR WEIGHING THE ANHYDROUS AMMONIA

determined, after which the valve *G* is opened, and the anhydrous ammonia is allowed to flow from the tank through the throttle valve or cock *H* into the refrigerator. During the time that the anhydrous ammonia is allowed to flow from the tank *B* through the throttle valve or cock *H*, the second tank, *K*, similar in construction to *B*, which is connected to the pipes *I* and *J*, is being filled.

21 In setting up the apparatus, care must be taken that the horizontal pipes, *C*, *K*, *I*, and *J*, leading to the two tanks, are long enough to allow sufficient flexibility to insure the proper working of the scales. Care must be taken also that the pipes *I* and *K* are so connected that no liquid ammonia can enter them, while the tanks for weighing the ammonia are being emptied. The liquid ammonia

receiver must be large enough to allow the level of the liquid to be carried at all times well below the inlets of the pipes *I* and *K*. The tanks *B* and *K* may be covered with a non-conductive covering to diminish the heating or cooling effect of the atmosphere on them. There should be little or no tendency to evaporate the liquid ammonia or to condense the ammonia vapor in the tanks *B* and *K*, and that such is the case may be determined by allowing them to stand for some time with the vent pipes open to the ammonia receiver *A*, and noting whether they gain or lose in weight. Tests should also be made to make sure that the valves used in the apparatus do not leak when they are closed off.

22 This method of weighing the ammonia will be cumbersome where large machines are to be tested and the Committee will be glad to receive suggestions regarding other methods which may be pursued in such cases.

ACTUAL REFRIGERATING CAPACITY.

23 In determining the actual refrigerating capacity of the machine the conditions must be those specified in the contract. For example, if a machine is guaranteed to produce a certain tonnage of refrigeration in cooling a storehouse in summer weather, the test should be made in the summer, if possible, or the capacity of the coils, which are used for refrigerating the various rooms, may be tested by employing relatively warmer brine. If the heat given to the brine is then not sufficient, a heater may be used to raise the temperature of the return brine the required amount. Such a heater may be readily constructed of a coil through which the brine passes, which is immersed in steam, so that the required amount of heat is added to the brine.

SPECIFIC HEAT OF BRINE USED

24 In all cases where the actual refrigerating effect is measured by the cooling produced in the brine circulated, the specific heat of the brine should be determined. For a description of an apparatus and the method of doing this, see article by Professor Denton, Transactions, Volume 12, p. 378.

TEMPERATURE AND PRESSURE OF AMMONIA GAS LEAVING REFRIGERATING COILS

25 It is necessary in computing the commercial refrigerating capacity from the weight of anhydrous ammonia circulated that the pressure and the temperature of the gas leaving the refrigerator be

known. As the pressure of the gas leaving the refrigerator is nearly that existing in the refrigerator, it may be taken as such without sensible error, and it is so taken in the tables given at the end of this report. Unless the gas leaving the refrigerator is superheated, there may be some liquid anhydrous ammonia leaving the refrigerator coils along with the gas. A thermometer at this point is necessary in all tests, because if any liquid ammonia leaves the refrigerator the calculated results will be too great and the machine will be doing less refrigeration than indicated by the measured amount of ammonia circulated.

TEMPERATURE OF THE ANHYDROUS AMMONIA AT THE EXPANSION VALVE

26 It is necessary in computing the commercial tonnage capacity that the temperature of the anhydrous ammonia be known on the high pressure side of the expansion valve. A thermometer well should be inserted in the pipe for this purpose.

COMPUTATION OF THE COMMERCIAL TONNAGE CAPACITY

27 The commercial tonnage capacity should be computed from the formula

$$R = \frac{W}{12{,}000} [L_2 - q + c_p (t_1 - t)] \qquad [1]$$

where R = commercial tonnage capacity or the tons of ice melting capacity per 24 hours.

W = weight of anhydrous ammonia evaporated in the refrigerating coils in pounds per hour.

L_2 = total heat above 32 degrees F. of one pound of the saturated ammonia gas at the pressure of the refrigerator.

q = sensible heat above 32 degrees F. contained in one pound of the liquid ammonia at the temperature observed before it passes through the expansion valve.

c_p = the specific heat of ammonia gas at constant pressure or 0.51.

t_1 = the temperature of the superheated ammonia gas leaving the refrigerator in degrees F.

t = the temperature corresponding to the pressure at which the ammonia gas leaves the refrigerator in degrees F.

28 Some tables of the properties of ammonia do not give the total heat and the heat of the liquid. The specific heat of liquid anhydrous ammonia is very nearly unity, and if taken at this figure we obtain equation [2] for computing the commercial tonnage capacity with the data given in such tables.

$$R = \frac{W}{12{,}000} [H_2 - (T_1 - T_2) + c_p \ (t_1 - t)] \qquad [2]$$

where H_2 = latent heat of evaporation of one pound of anhydrous ammonia at the pressure of the refrigerator.

T_1 = temperature of anhydrous ammonia observed just before it passes through the expansion valve in degrees F.

T_2 = temperature corresponding to the pressure of the ammonia gas in the refrigerator in degrees F.

and where the remainder of the notation is the same as in equation [1].

PURITY OF THE ANHYDROUS AMMONIA

29 In determining the commercial tonnage capacity it is necessary to make sure that the anhydrous ammonia is pure. In the case of absorption machines, there is usually some water present in the ammonia. The quantity of water should be determined. The method of doing this will be reported later on.

METHOD OF ALLOWING FOR ANY WATER PRESENT IN THE AMMONIA

30 (The Committee will report on this later on.)

COMPUTATION OF THE ACTUAL REFRIGERATING CAPACITY

31 The actual refrigerating capacity should be computed from the formula:

$$R' = \frac{W'c}{12{,}000} \ (t_2 - t_3) \qquad [3]$$

where R' = actual tonnage capacity, or the tons of ice melting capacity per 24 hours.

W' = weight of refrigerating fluid circulated per hour.

c = specific heat of the refrigerating fluid for the range of temperatures existing in the tests.

t_2 = temperature of refrigerating fluid returned to the machine.

t_3 = temperature of refrigerating fluid leaving the machine.

INDICATOR CARDS, ETC., IN COMPRESSION MACHINE

32 Indicator cards should be taken from the steam and ammonia cylinders of a compression machine. Thermometer wells should be placed in the inlet and exit ammonia pipes of a compressor, and the temperatures observed.

STRENGTH OF LIQUORS IN ABSORPTION MACHINE

33 The density of the strong and weak liquors should be determined in testing an absorption machine. It is essential in doing this that no gas be allowed to escape from the liquors on drawing from the machine. The liquors should be drawn off through a pipe which is surrounded with cold brine or some other refrigerant, and the density should be determined at a temperature at which there is practically no evaporation.

HEAT BALANCE

34 A balance should be made of the various quantities of heat received and rejected by a machine. This is important as proving the accuracy of a test.

TABLES

35 Five tables are submitted for reporting the results of tests. Table 1 gives the essential data and results for a test to determine the Commercial Tonnage Capacity. Table 2 gives more complete data for a test of a compression machine, and Table 3 the same for a test of an absorption machine. Tables 4 and 5 indicate the method of obtaining the heat balance for compression and absorption machines, respectively.

TABLE 1

DATA AND RESULTS FOR RECORDING THE COMMERCIAL TONNAGE CAPACITY

(All other data eliminated)

Arranged by the Committee appointed by the American Society of Mechanical Engineers, Code of 1907.

1	Duration of test	hours
2	Anhydrous ammonia evaporated per hour in the refrigerating coils (W)	lbs.
3	Average condenser pressure above atmosphere, or gage, pressure (made as near 168 lbs. per sq. inch above the atmosphere as possible)	lbs. per sq. in.
4	Average refrigerator pressure above atmosphere, or gage pressure (made as near 15 lbs. per square inch above the atmosphere as possible)	lbs. per sq. in

5 Average temperatur of liquid ammonia on high pressure side of the throttling valve or cock (T_1) ..... deg. F.
6 Average temperature of the ammonia gas leaving the refrigerator (t_1) ..... deg. F.
7 Temperature of saturated ammonia gas corresponding to the average refrigerator pressure (T_2) ..... deg. F
8 Total heat above 32° F. of one pound of saturated ammonia gas at the average refrigerator pressur (L_2) ..... B.t.u.
9 Sensible heat above 32° F. contained in one pound of liquid ammonia at the temperature observed before it passes through the throttle valve or cock (q) ..... B.t.u.
10 Commercial tonnage capacity =

$$\frac{W}{12{,}000}\ [L_2 - q + c_p\ (t_1 - t)]$$

or tons

$$\frac{W}{12{,}000}\ [H_2 + (T_1 - T_2) + c_p\ (t_1 - t)]$$

(See notation given in paragraph 27, t may without sensible error be taken equal to T_2, see paragraph 25.)

TABLE 2

DATA AND RESULTS OF TEST OF AN AMMONIA COMPRESSION MACHINE

Arranged in accordance with the form advised by the Committee appointed by the American Society of Mechanical Engineers, Code of 1907.

1 Made by ..... of .....
on machine located at .....
to determine .....
2 Type of engine driving compressor .....
3 Type of ammonia cylinders, single or double acting.....
4 Name of builders.....
5 Data regarding steam cylinders h.p.cyls. l.p.cyls.
(*a*) Number of cylinders .....
(*b*) Vertical or horizontal .....
(*c*) Cylinder dimensions in inches:
Bore .....
Stroke .....
Diameter piston rod .....
Diameter tail rod .....
(*d*) Percentage of clearance { head end ..... / crank end .....
6 Data regarding ammonia cylinders:
(*a*) Number of cylinders .....
(*b*) Single or double acting .....
(*c*) Vertical or horizontal .....
(*d*) Cylinder dimensions in inches:
Bore .....
Stroke .....
Diameter piston rod .....
Diameter tail rod .....
(*e*) Percentage of clearance.

7	Water wetted surface of condenser coils	sq. ft.
8	Brine wetted surface of refrigerator coils	sq. ft.
9	Date of trial	
10	Duration of test	hours

HOURLY QUANTITIES

11	Anhydrous ammonia evaporated in refrigerating coils	lbs.
12	Brine circulated	lbs.
13	Condensing water	lbs.
14	Water circulated through compressor jackets	lbs.
15	Steam consumed by engines driving compressor	lbs.
16	Steam consumed for all purposes, including auxiliary pumps or other machinery necessary to the plant	lbs.

PRESSURES AND TEMPERATURES (corrected)

Averages for test except where marked with an asterisk.

17	Condenser pressure, above atmosphere or gage pressure	lbs. per sq.in.
18	Refrigerator pressure, above atmosphere or gage pressure	lbs. per sq.in.
19	Temperature of liquid ammonia just before expansion valve	deg. F.
20	Temperature of ammonia gas leaving refrigerator	deg. F.
21	Temperature of brine, entering brine cooler	deg. F.
22	Temperature of brine leaving brine cooler	deg. F.
23	Range of temperature of brine	deg. F.
24*	Temperature of brine at beginning of test	deg. F.
25*	Temperature of brine at end of test	deg. F.
26	Temperature of condensing water supplied to condenser	deg. F.
27	Temperature of condensing water leaving the condenser	deg. F.
28	Temperature of air at condenser, dry bulb thermometer	deg. F.
29	Temperature of air at condenser, wet bulb thermometer	deg. F.
30	Temperature of cooling water supplied to jackets	deg. F.
31	Temperature of cooling water leaving jackets	deg. F.
32	Temperature of gas entering the compressors	deg. F.
33	Temperature of gas leaving the compressors	deg. F.
34	Temperature of engine room	deg. F.
35	Barometer, actual reading not corrected to 32 degrees F.	in.

DATA RELATING TO AMOUNT OF REFRIGERA ION RODUCED

36	Cooling effect per pound of anhydrous ammonia circulated $=L_2 - q + c_p (t_1 - t)$ in equation [1] or $H_2 + (T - T_2) + c_p (t_1 - t)$ in equation [2]	B.t.u.
37	Density of brine	
38	Specific heat of brine	
39	Total weight of brine in system (used when there is a correction for difference in temperature of the brine at the beginning and ending of the test)	lbs.

TONNAGE

40	Tonnage capacity computed from the amount of ammonia circulated. This is the commercial tonnage capacity if head pressure of ammonia gas is 168 pounds per square inch above the atmosphere in the test and the back pressure 15 pounds per square inch above the atmosphere	tons
41	Actual tonnage capacity computed from the weight of brine circulated and corrected for any difference in the temperature in the brine at the beginning and ending of the test	tons

CONDENSING AND CIRCULATING WATER

42	Total cooling water for all purposes in gallons per minute per ton of refrigeration produced in 24 hours	gals.

STEAM ENGINE AND COMPRESSOR DATA

43	Average revolutions per minute	revs.
44	Pressure of steam near engine throttle above atmosphere	lbs. per sq. in.
45	Indicated horse power of steam cylinders	h.p.
46	Indicated horse power of ammonia cylinders	h.p.
47	Friction in per cent of steam horse power	h.p.
48	Steam consumed by main engine per hour per horse power	lbs.
49	Steam consumed by main engine and auxiliaries per hour per horse power of the main engine	lbs.
50	Refrigeration in pounds of ice melting capacity per hour per horse power of main engine	lbs.

BOILER DATA

51	Kind of coal burned	
52	Average pressure of steam above atmosphere	lbs. per sq. in.
53	Average amount of superheating, if any	deg. F.
54	Average temperature of feed water	deg. F.
55	Water actually evaporated per pound of coal (determined by a test)	lbs.

ECONOMY

56	Ice melting capacity in pounds per pound of coal, based on the actual evaporation of the boiler	lbs.
57	Ice melting capacity in pounds per pound of coal, assuming that one pound of coal imparts 10,000 B.t.u. to the boiler	lbs.

Note: The data given in this table will be supplemented by a detailed description of the machine, giving type of condenser, refrigerator, etc., and arrangement of auxiliary pumps and other apparatus.

TABLE 3

DATA AND RESULTS OF TEST OF AN AMMONIA ABSORPTION MACHINE

Arranged in accordance with form advised by the Committee appointed by the American Society of Mechanical Engineers, Code 1907.

1 Made by ..... of .....
on machine located at .....
to determine .....

2 Total exterior surfaces (*a*) Steam coils in generator ..... sq. ft.
(*b*) Cooling coils in absorber .... sq. ft.
(*c* Condenser coils ..... sq. ft.
(*d*) Refrigerator coils ..... sq. ft.
(*e*) Exchanger coils ..... sq. ft.
(*f*) Weak liquor coils ..... sq. ft.
(*g*) Rectifier coils ..... sq. ft.
(*h*) Heater for strong liquor..... sq. ft.

Date of trial.....

3 Duration of test ..... } hours

HOURLY QUANTITIES

4 Anhydrous ammonia evaporated in refrigerating coils .... lbs.
5 Brine circulated per hour ..... lbs.
6 Cooling water used in condenser ..... lbs.
7 Cooling water ..... lbs.
8 Total cooling water used (the same as that used by the condenser if all the cooling water finally passes to the condenser) ..... lbs.
9 Steam consumed by generator ..... lbs.
10 Steam for all purposes other than for the generator ..... lbs.

PRESSURES AND TEMPERATURES (corrected)

Average for test except when marked with an asterisk.

11 Condenser pressure, above atmosphere, or gage pressure .. lbs. per sq. in.
12 Refrigerator pressure, above atmosphere, or gage pressure.. lbs. per sq. in.
13 Temperature of liquid ammonia just before expansion valve ..... deg. F.
14 Temperature of ammonia gas leaving refrigerator ..... deg. F.
15 Temperature of brine entering brine cooler ..... deg. F.
16 Temperature of brine leaving brine cooler ..... deg. F.
17 Range of temperature of brine ..... deg. F.
18* Temperature of brine at beginning of test ..... deg. F.
19* Temperature of brine at end of test ..... deg. F.
20 Temperature of cooling water supplied to the condenser ... deg. F.
21 Temperature of cooling water leaving the condenser ..... deg. F
22 Temperature of air at condenser, dry bulb thermometer .. deg. F.
23 Temperature of air at condenser, wet bulb thermometer .. deg. F.
24 Temperature of cooling water supplied to the absorber.... deg. F.
25 Temperature of cooling water leaving the absorber ..... deg. F.

STRENGTH OF AMMONIA, LIQUORS, ETC.

26	Density of strong liquor by Beaumé scale	deg.
27	Density of weak liquor by Beaumé scale	deg.
28	Temperature of room at a point between the generator and the absorber	deg. F.
29	Barometer, actual reading not corrected to 32 degrees F.	in.

DATA RELATING TO THE AMOUNT OF REFRIGERATION PRODUCED

30	Purity or strength of the anhydrous ammonia supplied to the refrigerator	per cent
31	Cooling effect per pound of anhydrous ammonia circulated $= L_2 - q + c_p (t_1 - t)$ in equation [1] or $H_2 + (T_1 - T_2) + c_p (t_1 - t)$ in equation [2] and corrected for any water in the ammonia supplied to the refrigerator	B.t.u.
32	Density of brine	
33	Specific heat of brine	
34	Total weight of brine in system (used when there is a correction for difference in temperature of the brine at the beginning and ending of the test)	lbs.

TONNAGE

35	Tonnage capacity computed from the amount of ammonia circulated = commercial tonnage capacity if head pressure of ammonia gas is 168 pounds per square inch above the atmosphere in the test and the back pressure 15 lbs. per square inch above the atmosphere	tons
36	Actual tonnage capacity computed from the weight of brine circulated and corrected for any difference in the temperature of the brine at the beginning and ending of the test	tons

CONDENSING AND CIRCULATING WATER

37	Total cooling water for all purposes, in gallons per minute per ton of refrigeration produced in 24 hours	gals.

GENERATOR DATA

38	Pressure of steam furnished to the generator, above atmosphere	lbs. per sq. in.
39	Temperature of condensed steam on leaving generator	deg. F.
40	Heat given up by one pound of steam to the generator	B.t.u.

BOILER DATA

41	Kind of coal burned	
42	Average pressure of steam above atmosphere	lbs. per sq. in.
43	Average amount of superheating, if any	deg. F.
44	Average temperature of feed water	deg. F.
45	Water actually evaporated per pound of coal (determined by a test)	lbs.

ECONOMY

46 Ice melting capacity in pounds per pound of coal, based on the actual evaporation of the boiler..... lbs.

47 Ice melting capacity in pounds per pound of coal, assuming that one pound of coal imparts 10,000 B.t.u. to the boiler; actual feed water temperature used in computing this figure..... lbs.

Note: The data given in this table should be supplemented by a detailed description of the machine, giving type of condenser, refrigerator, etc., and arrangement of auxiliary pumps and other apparatus.

TABLE 4

HEAT BALANCE FOR COMPRESSION MACHINE

All quantities computed in B.t.u. per hour

1 Refrigerating effect, to be based on either the weight of ammonia evaporated in the refrigerator or on the actual refrigeration.

(*a*) Based on the weight of ammonia evaporated .....

(*b*) Based on the actual refrigeration .....

2 Heat equivalent to work of compressing ammonia,—horse power of ammonia cylinders x 2545.....

3 Sum of items 1 and 2.....

4 Heat imparted to condenser water .....

5 Heat imparted to jacket water .....

6 Sum of items 4 and 5.....

7 Item 3 – Item 6 .....

Item 7 should be a small quantity, relatively, to the others. If a more exhaustive heat balance is desired the amounts of heat radiated and absorbed by the various parts of the machine may be estimated by computation, and the heat unaccounted for may be determined by comparing the algebraic sum with item 7.

TABLE 5

HEAT BALANCE FOR ABSORPTION MACHINE

All quantities computed in B.t.u. per hour

1 Refrigerators effect to be based on the weight of ammonia evaporated in the refrigerator or on the actual refrigeration.

(*a*) Based on the weight of ammonia evaporated.....

(*b*) Based on the actual refrigeration.....

2 Heat imparted by steam to generator, and to strong liquor if the latter is used.....

3 Sums of items 1 and 2 .....

4 Heat imparted to cooling water used by condenser and for cooling the weak liquor coils.....

5 Heat imparted to cooling water used by absorber.....

6 Sum of items 4 and 5.....

7 Item 3 – Item 6.

Item 7 should be a small quantity, relatively, to the others. If a more exhaustive heat balance is desired the amounts of heat radiated and absorbed by the various parts of the machine may be estimated by computation, and the heat unaccounted for may be determined by comparing the algebraic sum with item 7.

SPECIFIC HEAT OF SUPERHEATED STEAM

By A. R. DODGE, SCHENECTADY, N. Y.
Non-Member

1 The desirability of using superheated steam in modern power plants has necessitated an extension of steam tables into the region of superheat. In the determination of these tables the specific heat (c_p) or the gain in heat units per degree rise in temperature—the pressure remaining constant—is the most necessary element.

2 The first determination of the specific heat of superheated steam was made by Regnault at steam pressures ranging from 30 to 90 pounds, in which he found the value to be 0.48 irrespective of temperature. Until recently this value has been used for all pressures and temperatures.

3 During the past five years at least fifteen investigators have published their results,[1] giving a wide range of values. Seven state that, for the same pressure, c_p increases with increasing temperature. Seven find it constant irrespective of temperature. Three hold that it increases only as the temperature decreases, but nearly all agree that c_p increases as the pressure increases.

4 In 1901 the writer began investigations of the value of c_p; these tests have been continued up to the present time, and two of the methods employed are presented in this paper:

a The water injection method, using large quantities of superheated steam, averaging 10,000 pounds of steam per hour, which is reduced to a lower superheat by injecting water.[2]

b The throttling calorimeter method, employing superheated steam in both high and low pressure chambers of the calorimeter.[3]

To be presented at the Indianapolis Meeting (May, 1907) of The American Society of Mechanical Engineers, and to form part of Volume 28 of the Transactions.

[1] See Appendix.

[2] Tests made and results worked up with the assistance of Mr. J. V. Thomas.

[3] Tests made and results worked up with the assistance of Mr. H. H. Mapelsden.

WATER INJECTION METHOD—THEORY

5 If superheated steam is reduced to nearly a dry and saturated condition by the injection of a known amount of water, the heat balance gives the following equation, neglecting radiation:

$$W_1 (H_1 + c_{p_1}[T_1 sup - T_1 sat]) + Wq = W_2 (H_2 + c_{p_2}[T_2 sup - T_2 sat])$$

Where

W_1 = pounds of steam per hour from the boilers.
W = pounds of injection water per hour.
$W_2 = W_1 + W$ = the total flow per hour.
T_1 = the temperature of the steam at the initial condition.
T_2 = the temperature of the steam at the final condition.
q = the heat of the liquid of the injection water.
H_1 and H = the total heats of steam at the saturated conditions.
c_{p_1} = mean specific heat of steam at initial condition.
c_{p_2} = mean specific heat of steam at final condition.

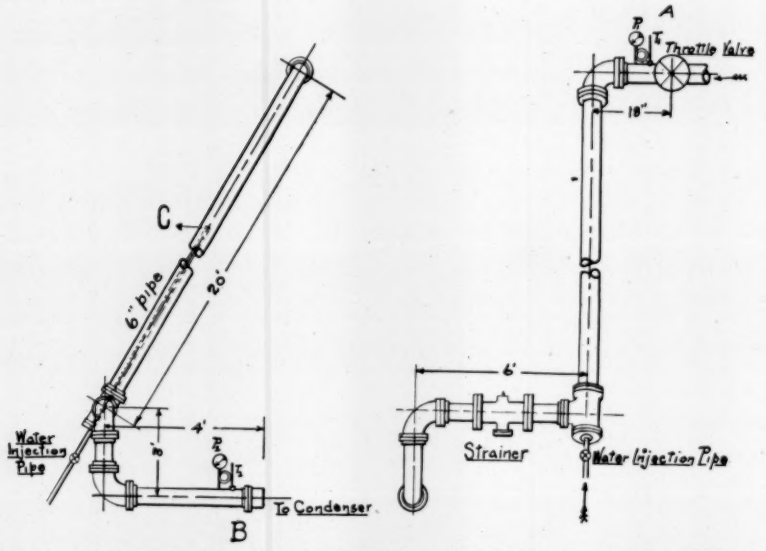


FIG. 1 ARRANGEMENT FOR MEASURING c_p BY WATER INJECTION METHOD

6 If we assume a value for c_{p_2}, we can solve the above equation for c_{p_1}. If the final superheat is slight, the error due to an incorrect assumption of c_{p_2}, is negligible. This is more than offset by the accuracy in determining the heat of the steam at its final condition when slightly superheated rather than when moisture is present.

DESCRIPTION OF APPARATUS

The general arrangement of piping is shown in Fig. 1.

7 The steam pressure was held constant on a 6 inch pipe line by means of a throttle valve A, while the superheat was kept as nearly constant as possible by introducing more or less saturated steam into the supply of superheated steam some distance back of the throttle valve A.

8 Water under a pressure about double that of the steam was forced into the steam at C in the form of a spray, counter-current to the direction of steam flow.

9 The final temperature was observed at B about 35 feet from the throttle valve A, the steam being thoroughly mixed after the water was added by means of elbows and a strainer in the pipe.

10 The initial temperature and pressure were taken near A, as indicated in the sketch.

11 The piping was heavily lagged, although the large steam flow eliminated errors due to radiation.

12 After the final temperature was taken, the steam was passed through an orifice in the pipe, thereby keeping the rate of steam flow constant. Each test was continued under constant conditions for a period of about one hour.

13 The steam was condensed in a surface condenser and weighed on calibrated scales at six minute intervals. The amount of water injected into the steam was also carefully determined by actual weight at frequent intervals.

14 The range of pressure covered by these tests was from 10 pounds absolute to 190 pounds absolute, and the test at each pressure included different degrees of initial superheat.

15 The initial and final temperatures were observed by means of mercury thermometers immersed in wells filled with mercury, and corrections were applied for the length of mercury column exposed above the well.

16 Each thermometer used was carefully calibrated in a drum supplied with wet steam held at any desired pressure up to 600 pounds. The correct saturated steam temperature for each pressure in the drum was determined by thermometers calibrated by the Bureau of Standards at Washington.

17 All pressures above atmosphere were observed by spring gages calibrated before and after each test by means of a standard dead weight tester. For pressures below the atmosphere mercury U-tubes were provided.

DISCUSSION OF RESULTS

18 As H_1 is practically equal to H_2, owing to the small drop in pressure, there is no error due to incorrect values of the total heat from steam tables, which is not true of most methods used in the determination of specific heat in which the pressure drop is considerable. These results are tabulated in Table 1, and plotted in curve

TABLE 1

CORRECTED RESULTS OF TESTS REDUCING THE TEMPERATURE OF SUPERHEATED STEAM BY THE INJECTION OF WATER

Test no.	Pressure at *A* lbs. abs.	Pressure lbs. abs. at *B*	Superheat at *A*	Superheat at *B*	Average Cp at init. press.	Date 1905	Lbs. hr. total steam cond.	Duration test in minutes under const. conditions.	Water injected into pipe lbs. hr.
	A	B	C	D	E	F	G	H	I
1	109.5	105.5	167.8	32.0	0.48	Aug. 4	11,447	17	557
2	110.2	105.5	150.6	23.2	0.49	Aug. 4	11,447	10	557
3	110.0	105.2	218.4	32.3	0.496	Aug. 4	11,447	9	800
4	186.7	185.7	156.5	17.3	0.495	Aug. 10	12,352	15	662
5	179.7	178.7	186.2	17.1	0.487	Aug. 10	12,352	5	780
6	169.8	167.8	127.8	25.3	0.503	Aug. 27	11,266	30	436
7	170.1	168.0	126.2	25.2	0.507	Aug. 27	11,266	34	436
8	169.8	168.4	127.0	40.5	0.547	Aug. 27	11,266	58	382
9	167.8	165.4	203.0	48.0	0.547	Aug. 27	11,021	24	690
10	121.6	117.7	224.5	31.9	0.537	Aug. 27	11,600	41	910
11	50.1	48.6	164.9	22.1	0.502	Sept. 20	5,250	75	300
12	104.5	100.2	247.0	26.3	0.515	Sept. 26	9,500	50	829
13	134.0	124.7	232.3	29.1	0.523	Sept. 26	9,603	50	790
14	160.2	151.5	228.4	27.0	0.502	Sept. 26	8,684	45	677
15	35.6	30.2	253.8	27.4	.49	Sept. 27	3,915	45	340
16	53.4	47.2	261.5	25.7	.49	Sept. 27	5,942	90	500
17	80.7	74.5	251.6	22.1	.485	Sept. 27	7,878	35	680
18	80.1	74.1	123.6	26.5	.481	Sept. 28	7,780	60	280
19	152.7	148.6	114.3	28.1	.537	Sept. 28	8,515	30	300
20	88.6	84.6	271.0	20.0	.476	Jan. 11, '06	6,500	30	590
21	88.6	84.6	270.0	29.4	.465	Jan. 11	6,550	60	545
22	88.6	84.6	286.0	17.0	.432	Jan. 11	6,500	50	590
23	9.89	5.37	368.0	38.0	.497	Feb. 26	1,405	20	180
24	9.9	5.35	379.0	38.5	.497	Feb. 26	1,374	30	180
25	10.25	5 5	387.5	36.0	.497	Feb. 26	1,400	20	185
26	9.82	8.5	354.6	20.4	.49	Mar. 6	3,720	70	473

form in Fig. 2 showing the values of c_p obtained. A log of test No. 16 is also shown in curve form, Fig. 3, indicating the amount of variation in the readings of an average test.

19 This log, which is a fair average of the fluctuations incident to this method, shows that if it is extended over a considerable period of time, reliable values of specific heat may be derived.

20 This method indicates that the temperature has an inappreciable effect on the value of c_p, but that there is a regular increase of the value of specific heat as the pressure increases.

THE THROTTLING CALORIMETER METHOD USING SUPERHEATED STEAM ON BOTH HIGH AND LOW PRESSURE SIDES OF THE CALORIMETER

21 The general method of using the throttling calorimeter to determine the average value of c_p, is to supply the high pressure side

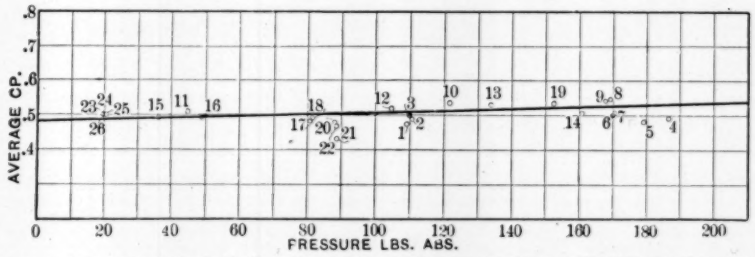


FIG. 2 WATER INJECTION TESTS UPON SUPERHEATED STEAM FOR DETERMINATION OF c_p

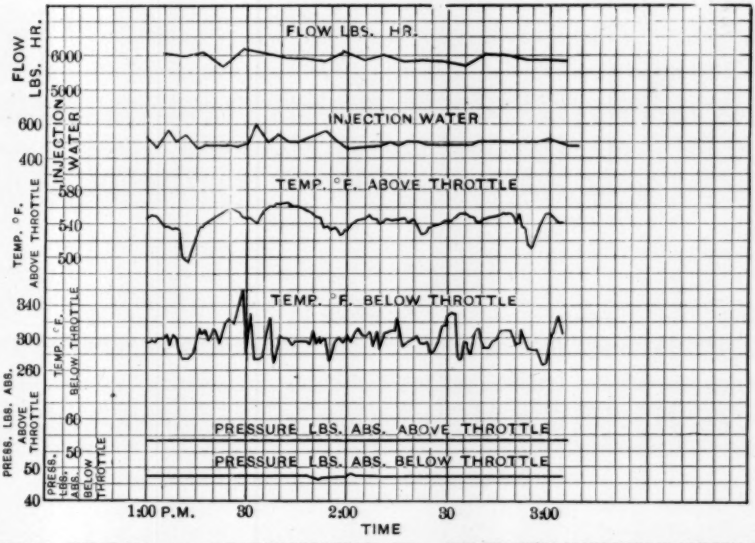


FIG. 3 TEST NO. 16. DETERMINATION OF c_p BY WATER INJECTION METHOD READINGS BETWEEN 1.35 P.M. AND 3.05 P.M. USED FOR CALCULATIONS

with steam from which practically all moisture has been eliminated. Neglecting radiation:

$$H_1 = H_2 + c_{p2}\,(T_2\,sup - T_2\,sat)$$

Where

H is the total heat.

T is the temperature in degrees F.

22 Values determined in this way by several authorities[1] show an increase of c_p with increasing temperature, but as has already been pointed out,[2] the values of total heat given in the steam tables are not sufficiently reliable to permit of accuracy in such determinations.

23 The following method, using the throttling calorimeter for determining the relation of specific heat to pressure and temperature, has not involved the use of the steam tables, and is, therefore, not open to errors from that source.

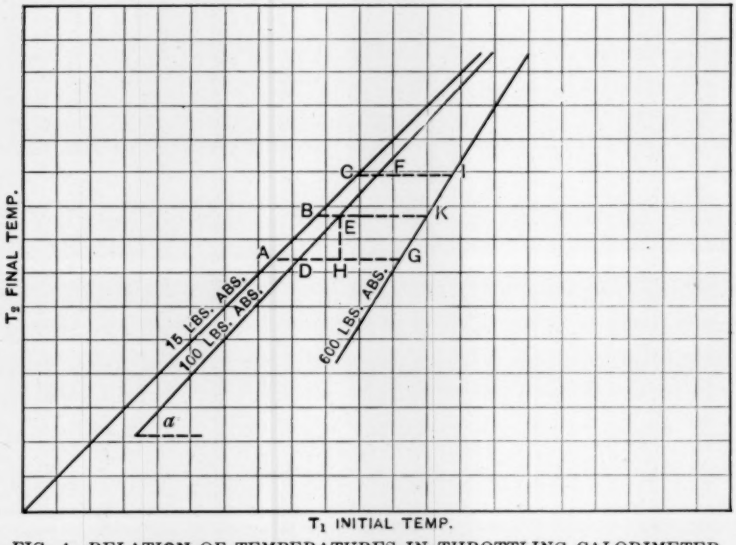


FIG. 4 RELATION OF TEMPERATURES IN THROTTLING CALORIMETER

THEORY

24 In a throttling calorimeter, if we assume no drop in pressure through the orifice and neglect radiation, T_1 would equal T_2. If we vary the initial temperature at a constant initial pressure of 15 pounds absolute we will have the relation between initial and final temperatures, as shown in the line A B C, Fig. 4, which represents the rise in temperature of the steam on both sides of the orifice without drop in pressure.

[1] See Appendix.

[2] Peake: Proceedings of Royal Institute, June 28, 1905, p. 201. Denton: "Stevens' Indicator," October, 1905, p. 383.

25 If we now increase the initial pressure from 15 pounds to 100 pounds by means of a proper sized orifice and sufficient rate of steam flow, and continue to keep the low pressure side of the calorimeter at 15 pounds absolute, we will obtain a series of temperature readings, D, E, and F, Fig. 4, on the high pressure side of the calorimeter corresponding to the simultaneous temperature readings A, B, and C respectively, obtained on the low pressure side.

26 If the points D, E, and F when plotted lie in a straight line, c_p equals $\frac{E\,H}{D\,H} = tan.\ a$, and will remain constant for any range of temperature which the line covers. This line $D\,E\,F$ represents therefore the rise in temperature obtained on the high pressure side of the orifice.

27 If we further increase the initial pressure to 600 pounds, keeping the final pressure at 15 pounds as before, we will obtain the

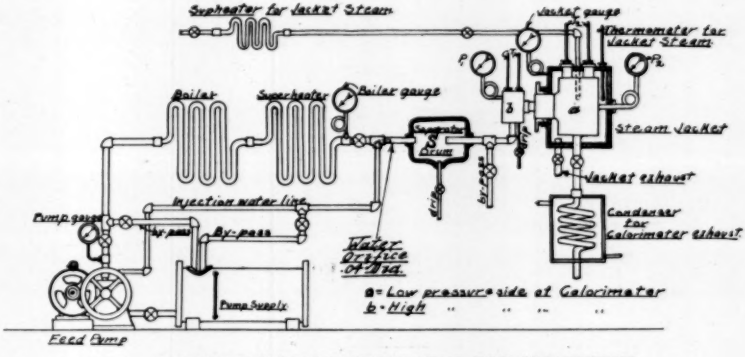


FIG. 5 ARRANGEMENT OF APPARATUS FOR MEASURING c_p

initial temperature readings G, K, and I, corresponding to the final simultaneous temperature readings A, B, and C, respectively.

28 If these points G, K, and I also lie in a straight line, the specific heat will be constant for the temperature range between G and I on this 600 pound line.

29 A variation in the value of c_p, therefore, for the lines $D\,E\,F$ and $G\,K\,I$ will be caused only by the variation in pressure, namely, from 100 pounds to 600 pounds, and the ratio of the tangents of the angles formed by these lines with the horizontal will represent the variation in c_p.

30 The results obtained in tests of superheated steam in the throttling calorimeter here described indicate that from 15 pounds to 600 pounds pressure, covering a range of temperature of several

hundred degrees, there is no deviation from a straight line when plotted as in Fig. 4. Consequently the above theory indicates that c_p does not vary with temperature within these limits.

DESCRIPTION OF APPARATUS

31 The general arrangement of boilers, calorimeter, etc., used in these tests is shown in Figs. 5 and 6. This calorimeter was designed after two years of experimental work, during which improvements were made to eliminate radiation, conduction, thermometer errors, temperature lag, and errors in temperature due to velocity of the steam jet. The water was forced by a high pressure pump into two flash boilers connected in series by means of which the superheat could be raised to any desired amount.

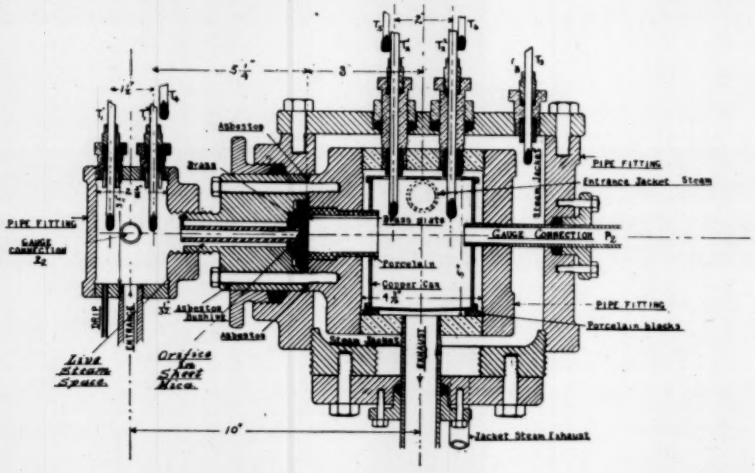


FIG. 6 THROTTLING CALORIMETER NO. 6 FOR c_p TEST 3

32 The steam was then passed to the separator drum S at which the temperature was controlled by injecting water similar to the method described previously. The pressure and temperature entering the high pressure side B of the calorimeter was held constant with practically no variation.

33 The low pressure side of the calorimeter was jacketed, and the steam in the jacket was held at nearly the same temperature as the exhaust from the calorimeter by means of a superheated steam supply at atmospheric pressure.

34 The steam passing through the calorimeter was weighed after being condensed, and Fig. 7 shows the relation between the absolute pressure on the high side and the steam flow.

35 Orifices of two sizes were used, the diameters of which were 0.052 inch and 0.1 inch. The object of using the larger orifice was to eliminate effect of possible radiation, although in addition to jacketing the low pressure chamber, the jacket was thoroughly lagged with magnesia. No different results were obtained, however, when using the larger orifice, indicating that radiation was negligible. The question of radiation was further investigated by measuring the drop in temperature in the steam from one chamber of the calorimeter to the other with the same steam flows as during the regular tests but with no drop in pressure as the orifice was entirely removed from the calorimeter. These tests indicate that there is a slight correction in spite of all precautions, but of such small limits that $\bar{c}_p$ could not be affected more than 1 per cent.

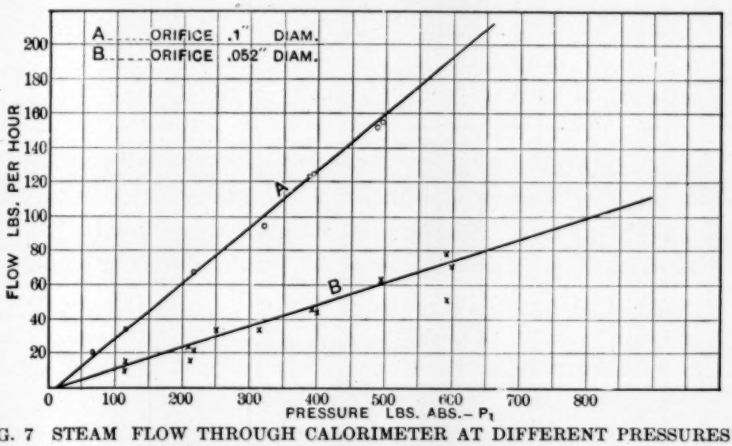


FIG. 7 STEAM FLOW THROUGH CALORIMETER AT DIFFERENT PRESSURES UNDER AVERAGE SUPERHEATED CONDITIONS

36 Each of the varying pressure tests, of which there are 85, were run from six to eight hours, each initial temperature point being held constant for about one hour.

37 All pressures on the high pressure side were held constant by means of spring gages calibrated before and after each test, using a reliable dead weight tester.

38 The pressure on the exhaust side of the calorimeter was held at 15 pounds absolute by means of a U-tube gage filled with mercury. The piping connecting this U-tube to the low pressure chamber was closed at the end and holes were drilled through the pipe to avoid a possible error from the velocity head of the steam jet through the orifice.

39 Tests were also made with 165 pounds absolute on the low pres-

sure side and the results are consistent with those having 15 pounds exhaust pressure and plotted in Fig. 8.

40 The temperature was held constant by means of a calibrated thermometer on the high pressure side. The stem of this thermometer was packed where it passed through the calorimeter wall and the bulb placed in direct contact with the steam. Another calibrated thermometer was also used in the high pressure chamber as a check. Stem corrections were applied in accordance with the following formula, which has been fully verified for such conditions:

$$\text{Correction} = (T_1 - T_2)\ (T_1 - T_3)\ 0.000087^\circ\ F.$$

Where

T_1 = observed reading of the chamber.
T_2 = reading just visible above chamber casing.
T_3 = temperature of exposed stem.

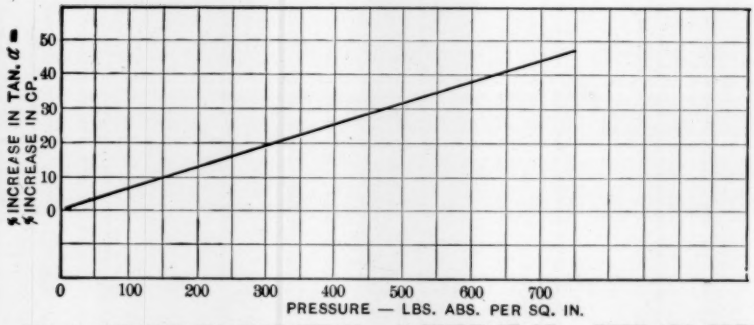


FIG. 9 SPECIFIC HEAT OF STEAM. % INCREASE OF c_p WITH ABS. PRESSURE FROM VALUE AT 15 LBS. ABS.

41 The thermometers were frequently calibrated in wet steam supplied to a drum at pressures up to 600 pounds. These pressures were determined and held at the correct value by a dead weight testing apparatus. The correct temperature at these pressures was determined by thermometers calibrated by the Bureau of Standards at Washington. The thermometers were also corrected for pressure on the bulb by the subtraction of 1 degree F. for each 100 pounds gage pressure, a correction determined by separate experiments in connection with these tests.

42 The thermometers on the low pressure side were also placed directly in the steam and properly packed to prevent leakage around the stem.

METHOD OF MAKING TESTS

43 Readings were not recorded until the apparatus was thoroughly heated up and conditions constant.

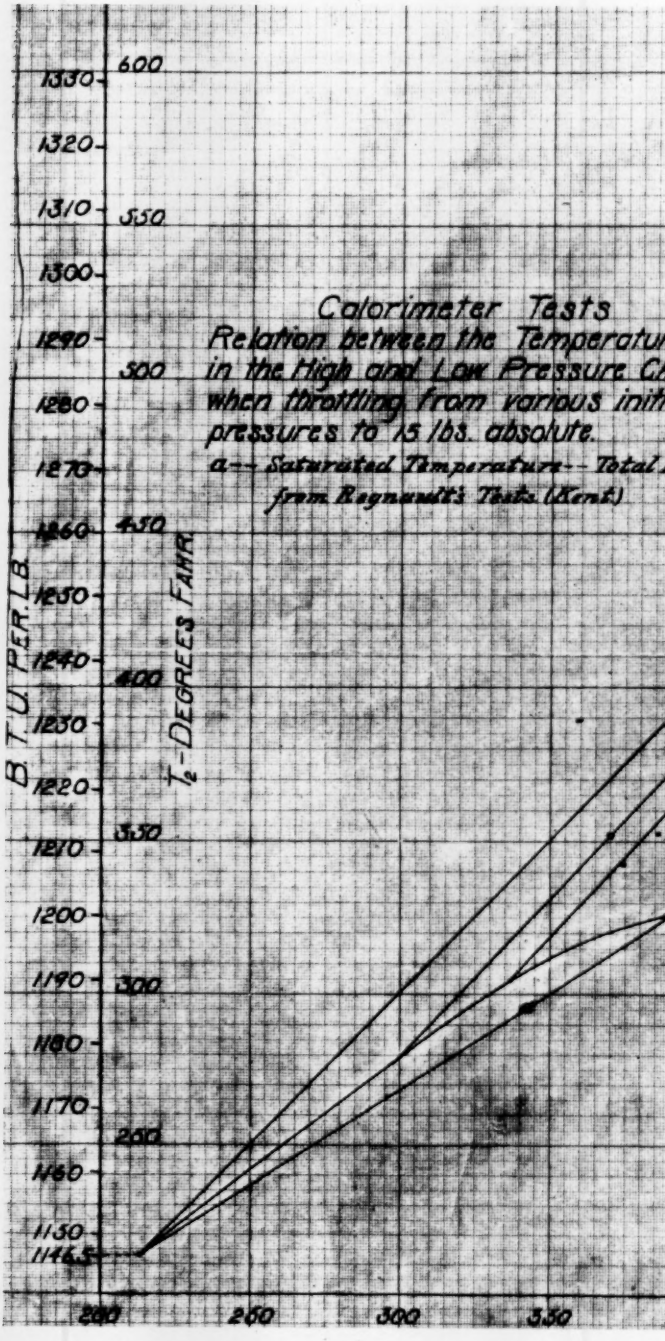
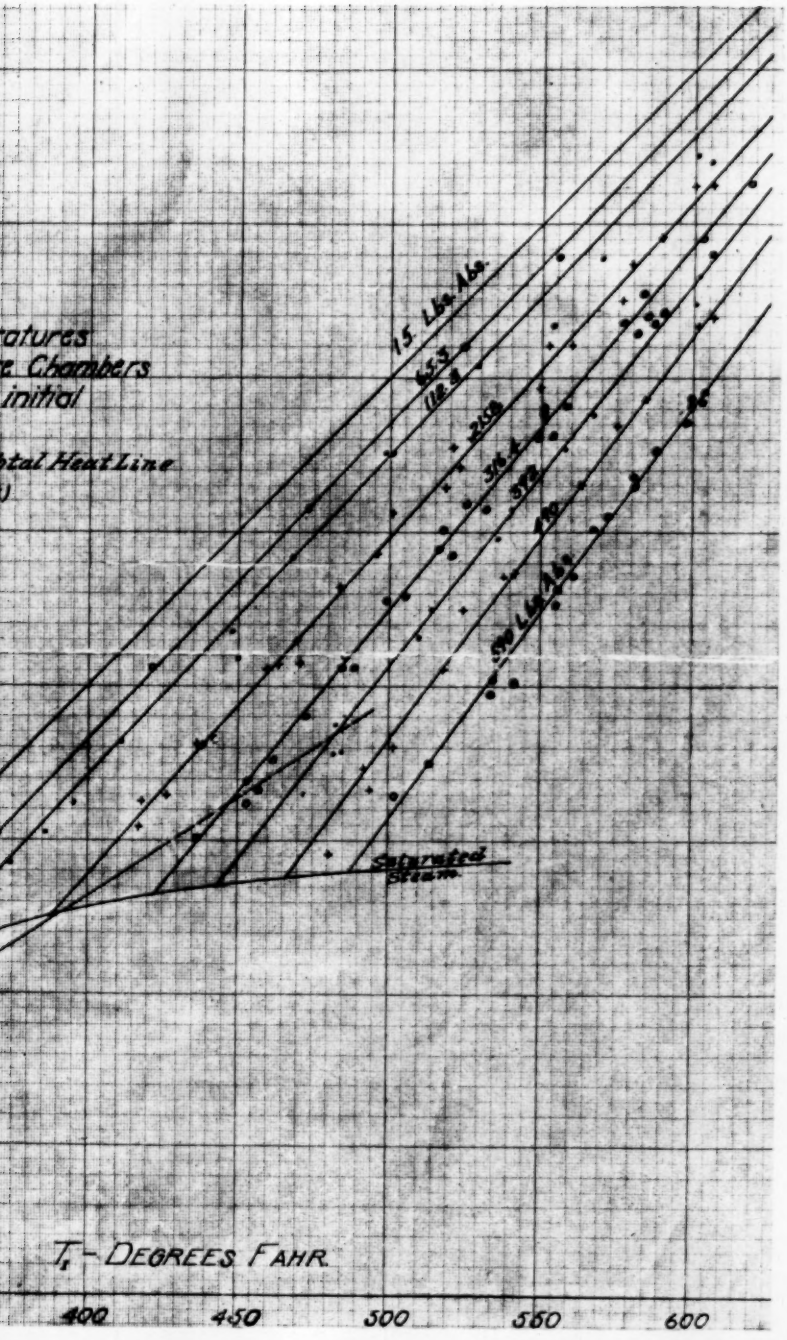


FIG. 8 TEST SHOWING RELATION BETWEEN TEM



TEMPERATURES IN HIGH AND LOW PRESSURE CHAMBERS

44 In order to prevent temperature lag, the initial conditions were held constant for an hour if necessary and frequent readings taken on the lower temperature T_2. The initial temperature could be controlled within one-half degree of the desired value. There was no variation in the initial and final pressures which were both controlled by throttling. After all conditions had remained constant over a satisfactory period the superheat on T_1 was changed and the operation was repeated the pressure conditions remaining unchanged. These observations are plotted in curve form in Fig. 8, and show clearly the convergence as well as the fact that the lines are straight. In other words the specific heat, as indicated by these tests, does not vary with temperature and increases with increasing pressure.

CONCLUSIONS

45 Fig. 9 shows the percentage increase of *tan.* α or c_p at different initial pressures from the value at 15 pounds absolute.

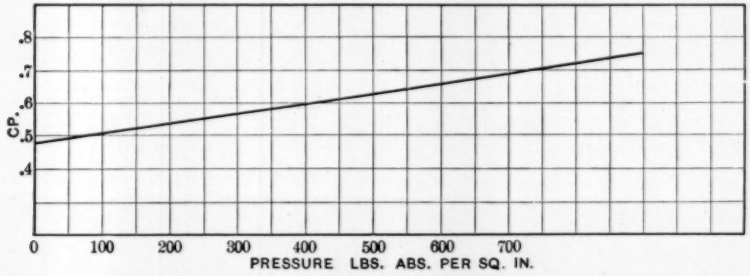


FIG. 10 VARIATIONS OF c_p WITH ABS. PRESSURE USING THROTTLING CALORIMETER TAKING $c_p = 0.48$ AT 15 LBS. ABS.

46 If we take the value of c_p at 15 pounds absolute to be 0.48, as determined by tests of Regnault and others and by the method of injecting water into superheated steam already described in this paper, and plot the percentage increase in this value for the various pressures as given in Fig. 9, we obtain the result shown in Fig. 10.

47 In Fig. 8 the B.t.u. per pound for superheated conditions are given as ordinates, using the values of c_p found in this paper and assuming Regnault's value of total heat at 15 pounds absolute to be correct. If we extend the constant pressure lines on this curve to the saturated condition T_1 and draw a saturation curve through these points, this curve will show the coincident values of total heat and temperature for all pressure conditions.

48 There is also plotted the total heat line taken from Regnault's steam tables; the dotted portion of this line is obtained from the table

given by Kent. The maximum variation involved in the two saturation curves is 1.6 per cent at a pressure of 600 pounds absolute. It shows the possibilities of error in determinations of c_p involving the use of steam tables.

SUMMARY

49 A comparison of the two methods here expressed as recorded in Figs. 2 and 10, shows approximately the same increase in the value of c_p with increasing pressure. The equation for this line is,

$$c_p = 0.4754 + 0.00031p$$

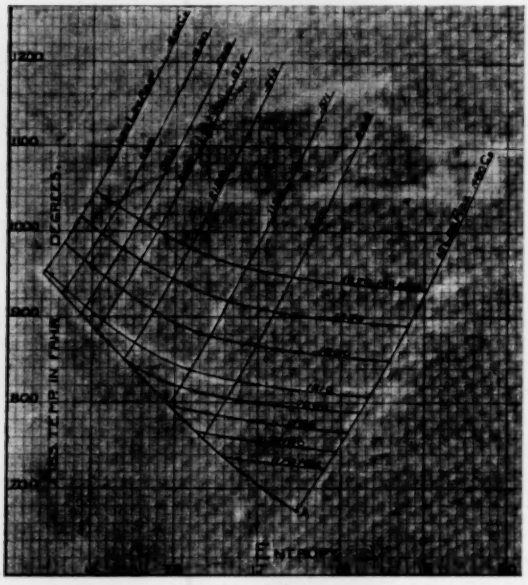


FIG. 11 ENTROPY DIAGRAM ASSUMING VALUES OF c_p AS DETERMINED IN THIS PAPER. POINT *A* TAKEN FROM STEAM TABLES

50 Both methods also indicate that at constant pressure the specific heat is constant for all ranges of temperature investigated.

51 The general form of the curves for constant values of c_p is best shown by the temperature entropy diagram plotted in Fig. 11, which also includes the curves of constant heat. This diagram is derived from the curved total heat-temperature line given in Fig. 8.

52 This investigation has been carried to pressures several hundred pounds above those of tests heretofore published and eliminates the uncertainty of methods which involve the assumption of exactly dry and saturated steam.

APPENDIX

TABLE 2

VALUE OF c_p AT ATMOSPHERIC PRESSURE BY VARIOUS AUTHORITIES

Author	Publication and date	Temp. °F.	c_p at atmos. pres.	Variation of c_p with increasing pressure	Variation of c_p with increasing temp.
SUPERHEATED STEAM COOLED BY WATER JACKETED CALORIMETER					
Regnault	Ann. de Chimie et de Physique, Tome 23.	Varied	0.4805	None	None
Carpenter (Jones)	Sibley Journal 5–1904	Varied	0.4844	Increases	None
THROTTLING CALORIMETER. SATURATED STEAM EXPANDED TO LOWER PRESSURE					
Grindley	Phil. Trans., Vol. 194	239	0.4317	Increases	Increases
Hirn				Increases	Decreases
Griessmann	Zeit. V. D. Ing., 52, 1903	269	0.506	Increases	Increases
Peake	Proc. Royal Soc. A-509, 1905	Varied	0.43	Increases	Increases
Carpenter (Stewart & Marble)	Sibley Journal, May, 1904	Varied	0.463	Increases	None
Carpenter (Hoxie & Wood)	Sibley Journal May, 1904	Varied	0.4825	Increases	None
Carpenter (Sickles)	Sibley Journal, May, 1904	Varied	0.48	Increases	None
SUPERHEATING STEAM ELECTRICALLY					
Peake	Proc. Royal Society A509, 1903	Varied	0.46	None	None
Carpenter (Berry)		Varied	0.48	Increases	None
Carpenter (Thomas)	Trans., A. S. M. E., Eng. Mag., Mar. '07	212	0.49		
		402	0.487	Increases	Decreases
Lorenz	Z. V. D. I., No. 20			Increases	Decreases
Knoblauch & Jakob	Enginering, L, Feb. 22, 1907	212	0.445		Decreases
		700	0.49	Decreases	then increases
FROM COMBUSTION OF EXPLOSIVE GASES					
Mallard & LeChatelier	Zeit. V. D. Ing., Tome 48	212	0.46	None	Increases
Sarran & Vieille	"	212	0.464	None	Increases
Langen	"	212	0.463	None	Increases

Author	Publication and date	Temp. °F.	c_p at atmos. pres.	Variation of c_p with increasing pressure	Variation of c_p with increasing temp.
		FROM CALCULATION			
Reeve	Wor. Poly. Journal	215	0.39	Increases	Increases
Hirn		265	0.4895	Increases	Decreases
Gray	London Engineer	236	0.38		
Zeuner			0.568		
Weyrauch	Zeit. V. D. Ing., Tome 48	212	0.468	Decreases	Increases
Perry	Steam Engine	212	0.36	Increases	None
Roentgen	Thermodynamics	Varied	0.4805	None	None
Wagner	Rose Technic, 1905	284.4	0.513	Increases	
Knoblauch,	Publication by authors	212	0.493	Increases	Decreases
Linde & Klebe	Berlin, 1905	356	0.479		

DISCUSSION

OUR PRESENT WEIGHTS AND MEASURES AND THE METRIC SYSTEM

By Henry R. Towne, Published in November Proceedings

Mr. H. R. Towne In reviewing the discussions of this paper I note two significant facts: (*a*) that in each case the argument is usually partisan, either strongly pro-metric or strongly anti-metric, and (*b*) that where anti-metric it deprecates further agitation because not only useless but dangerous. Both facts tend to strengthen my plea for the reference of the whole question at issue to a competent *technical commission.* No avowed partisan is qualified to adjudicate a disputed issue; no disputed issue can permanently be disposed of by the suppression of discussion. For eighty years the question of adopting the metric system in place of our present system of weights and measures has been under discussion without result, except the Act of July 28, 1866, legalizing the use of the metric system; previous failures have never deterred the advocates of the proposed change from returning to the attack, and will not deter them in the future; Congressional committees have been tested again and again, and have established their unfitness for solving a problem so vast in scope, so vitally important, and so inherently technical. Both sides desire a final decision. The leaders on each side should welcome a decision by a competent, but impartial tribunal, and should now join hands in an earnest effort to secure the creation of such a tribunal and the reference to it of the whole question at issue. To question our ability to secure a commission which shall be both competent and impartial is to question our ability for self-government. Doubts on this point may be dismissed as unjustified by all past experience and as unworthy of our people.

2 Nearly all of the arguments adduced in the discussions are based on engineering experience; some favoring, but more opposing, the adoption of the metric system, but it should be kept in mind that engineers, while vitally concerned, do not represent the only, nor even the chief interest involved; that each and all of our great industries,

including agriculture, mining, manufacturing, transportation, and commerce, by no means omitting science in all its branches, are equally concerned and equally entitled to consideration; and that, above all, the greatest interest involved is that of the plain people in the innumerable transactions of their daily life.

3 Perhaps the most regrettable note in the discussions is that of intemperateness; the implication in each argument that it is infallible; the attempt to belittle the metric system by calling it "the metric fallacy;" the disposition to see one side only of the case, and, in general, the absence of the judicial spirit. Not in this direction lies our best hope of a wise and lasting solution of the question. Mr. Dale says, "there can be no compromise"—but this may be the rallying cry of the pro-metricists as well as of the anti-metricists. What each should seek is a solution which shall be best not for any one interest but for *all interests*, for the American people, and which, being best for all, shall be final. In a case of this kind we should at least try to ascertain what measure of compromise may be available before rejecting all suggestion of compromise. The only such suggestion in the paper under discussion is that, to assist in a final choice between the metric and our present system, we should first ascertain what it may be feasible to do to simplify and improve the latter.

4 With Mr. Dale's eulogy of John Quincy Adam's Report of February 22, 1821, and his suggestion that it should be reprinted and widely distributed at government expense, I am in hearty accord, but I must dissent from his sweeping accusation that the National Bureau of Standards is "the center of a metric propaganda." Having recently visited the Bureau, and spent a long afternoon there with its able head, Professor S. W. Stratton, I am prepared to state that its action has been limited to preparing and issuing revised and accurate comparative charts of the two systems; that its splendid facilities are wholly being utilized in other work of the highest usefulness; and that its present chief executive will loyally accept and enforce any decision respecting our national system of weights and measures which may finally be made by whatever tribunal may be authorized to pass thereon. In much of the purely scientific work of research in which it is engaged, the Bureau finds it convenient to avail of the metric system, and therefore properly does so.

5 Mr. Webber's suggestion that the proposed Commission, if created, should be authorized to include in the scope of its investigations the question of slightly changing the basis of our monetary system, so as to bring it into harmony with the pound sterling and the franc, is interesting and pertinent. I can see no objections to this,

but many advantages. So also as to Mr. Oberlin Smith's suggestion that the Commission should be authorized to consider the feasibility and expediency of revising our system of notation—I can see no objection to thus enlarging the scope of the investigation, and it is conceivable that some good might result, even if only by deciding that such a change is impracticable. The question is thoroughly germane to the main issue, even if somewhat academic.

6 Mr. Suplee has made a valuable contribution to the discussion by giving the French law of July 4, 1837, which covers the final adoption of the metric system, and Mr. Halsey further illustrates the fact that in the so-called metric countries the old measures still remain those chiefly used by the common people in their every day transactions. Rear Admiral Melville, in an admirable review, expresses doubt as to the possibility of securing a non-partisan commission, but to admit this doubt would be to question our ability for self-government. The work of the Commission would be to investigate and report on facts; action on its findings would still rest with Congress, and the latter represents the people. As Mr. Christie points out, heretofore a "stable foundation" has been wanting for proposed legislation, and, it may be added, is equally needed as the basis for a decision to reject definitely the metric system. Still more is the work of the proposed Commission needed to formulate the possibilities outlined by Professor Kent of improvement in the details of our present system.

7 This discussion, like others preceding it, has shown an overwhelming preponderance of opinion among engineers against the adoption of the metric system, and also, but with less emphasis, a contrary view on the part of scientists and instructors. Each group undoubtedly knows what is best for its own purposes, but neither should seek to lay down the law for the other, nor forget that many other groups and interests are equally concerned and have an equal right to be heard.

8 It is true that the work of the engineer, especially that of the mechanical engineer, underlies all of our productive industries, and provides the implements, machines, and motive power by which they achieve their results, and that, therefore, the views of engineers, collectively, may be entitled to greater consideration than those of any other group, but those views, to prevail, must appeal to and be accepted by the many other interests concerned. A clearing house, for the free interchange of views and experience, is needed; can it be provided in any better way than by the proposed Technical Commission?

9 Another fact made clear by the discussion is that our present system of weights and measures is overlaid with many needless and some obsolete multiples and sub-multiples of its basic units; that it could easily be reconstructed on simpler lines without disturbing its basic units; that in doing this it could undoubtedly be decimalized largely if not completely; that in doing this also its units could probably be brought into more effective relations to each other; and that possibly their relation to the metric units, in some cases at least, might be made more simple and direct. Until these possibilities have been exhaustively studied and definitely formulated, it is impossible for anyone intelligently to pass judgment on the feasibility and expediency of modifying our present system, or of abandoning it in favor of the metric system. To do this would be the work of the proposed commission. In the accomplishment of that work it could and should be greatly aided by the coöperation of the National Bureau of Standards, the facilities of which should be placed at its command for that purpose. Constructive legislation, based on the findings of the Commission, would still rest with Congress.

10 In closing the discussion I beg to point to the fact that the agitation of this question has now continued for over eighty years; that it is as far from solution now as at the beginning that the agitation, as it is prolonged, becomes more acute, not less; and that all attempts to find a solution by means of Congressional Committees have proved abortive and fruitless. Therefore, I maintain, it is time that we find some better and more promising method whereby to study and to solve this important national problem. Is there any known method better than the creation of a special Technical Commission? If so, it should be presented and discussed. If not, that method should be availed of and should have the loyal support of all, whatever may be their individual opinions and preferences as to what should be the findings of the commission.

THE EVOLUTION OF GAS POWER

BY F. E. JUNGE, PUBLISHED IN NOVEMBER PROCEEDINGS

MR. FRANK FIRMSTONE Neither Mr. Junge nor Mr. J. E. Johnson gives the figures on which he bases his estimates of the relative consumption of "waste" gases per horse power in gas engines and by burning them under boilers to raise steam.

2 The most complete statement on the question seems to be in Mr. Adolph Greiner's paper in "Iron and Steel Inst.," No. 1, 1898, and No. 1, 1900. He there states (No. 1, 1898, p. 25) as the result of

experiments at Seraing, that steam machinery at the blast furnaces used 26.49 pounds of water per indicated horse power per hour equivalent to 22 cubic meters of gas. The first gas blowing engine (No. 1, 1900, p. 111) used on a careful trial 2.56 cubic meters of gas per indicated horse power per hour, or less than one-eighth of the average consumption of the steam machinery of various kinds as stated above.

3 On this basis, assuming steam engines using 16 pounds of water, which Mr. Johnson indicates as possible, the gas consumption for steam engine and boiler is: 26.49 : 11 :: 10 : 2 cubic meters of gas, or about four times the consumption actually realized by the Seraing blowing engine. A saving of two-thirds of the gas therefore seems a moderate assumption in this case, even admitting that the boilers might have been improved, on which point there is some information in Greiner's paper of 1898 (footnote, p. 25).

4 The writer saw the Seraing engines at work at Seraing, Dieffer-dingen, and at the Cochran & Ormsby Works at Middlesbrough in September and October, 1902, and also the gas engines of various makes at the Düsseldorf exhibition, then open, and engines at work at several iron works in that vicinity. As a result, previous favorable opinions were fully confirmed and today, if he were designing a blast furnace works to include several furnaces, he would, without hesitation, adopt gas engines rather than steam engines and boilers.

5 In the case of an isolated furnace, the considerations advanced by Mr. Johnson in favor of steam might perhaps turn the scale against the gas engine. The decision would hang on the question, "How long would it take to get gas from a gas producer in good order, and charged, ready to light, compared with the time needed to get good fires under three or four boilers already under steam with gas?" As to the first part of the question the writer has no experience. As to second, assuming that coal was at hand, and the grates in good order, as would be the case under efficient management, to get men together, to clear the bar of sand or ashes with which they must be covered to protect them when the boilers are using gas only, and, finally, for time to make the fires and let them come up an hour or an hour and a half is less time than would actually be expended in most cases.

MR. F. E. JUNGE In view of the great number of discussions advanced I will have to limit my reply to a short survey of those remarks which appear to contain some contradiction of my statements. For more detailed information on these questions I must refer those interested to my various publications in the technical press.

2 Dr. C. E. Lucke and R. E. Mathot. Dr. Lucke's discussion is

noteworthy and most vital to the question. I entirely agree with him that the limitations of the gas engine, and especially its relations to the steam engine, have not been sufficiently emphasized in my paper. Unfortunately, that portion of the paper which was devoted to the questions upon which he enlarges was cut out by the editing staff of the Society, probably because otherwise the paper would have been abnormally long

3 I can also quite understand that the situation as I presented it, basing my remarks on my familiarity with European conditions, and the remarkable performances which I recorded, must seem optimistic and even impossibly high to one who has limited his energies to the study and development of gas engines in America.

4 In this connection I find that the figures which Monsieur Mathot submits in his discussion are apt to convince those who may doubt the correctness of my statements that the efficiencies recorded have actually been attained on the European continent.

5 I believe that when commenting on the evolution of gas power it is logical, necessary, and proper to give, beside the average performances of gas prime movers, which were graphically represented by several curves, also the best figures which have so far been attained both with large and small engines, so long as they are clearly designated as maxima.

6 Mr. J. E. Johnson "The refinements which constitute the glory of mechanical engineering being utterly insignificant in the presence of the necessities of metallurgy, if considered from the commercial point of view." This is perhaps the most remarkable statement in Mr. Johnson's discussion. If it is meant to represent the general attitude of American iron masters, I feel at a loss to reply. It certainly stands in contradiction to the general modern tendency which prevails in European metallurgical circles. The fact that all iron smelting plants in Germany have equipped their furnaces with gas, instead of steam blowing engines will serve to substantiate my statement. However it is useless to enlarge on this question since time will give the final answer.

7 Also, I leave it to everyone to decide for himself as to whether the name "waste gases" was imparted to the gases emanating from the blast furnace when or because they were utilized, or because they were wasted. As to the technical argument, the question is this: What reserves are available for a blast furnace which has shown signs of weakness and must be closed down, thereby depriving the power section of the plant of a certain contribution or quantity of gas, which is indispensable for fulfilling all internal and external obliga-

tions that have been undertaken by the works management. It would be wrong to conclude that, owing to the remote possibility of having to close down one or the other furnace, the surplus gas emanating from them should not be utilized at all, except for covering the requirements at the furnace proper. On the contrary, in Europe engineers are endeavoring to reduce the unavoidable losses and the demand at the furnace, namely, leakage at furnace top, gas used for blowing and for heating the blast, to the lowest possible amount in order to reserve the rest for more profitable usage.

8 Considering the case that in a plant of four furnaces one is beginning to show signs of distress and must be shut down, then one-fourth of the total quantity of gas required is no longer generated. Of this amount only one-half must be replaced by suitable means, since almost 50 per cent of the total is used for operations at the blast furnace proper, and can be dispensed with when the furnace is not working. As for the rest, it is, of course, unnecessary to replace the total quantity of gas so long as we have sources of generation available which can deliver the same amount of kinetic gas energy to the power plant. This reserve energy can be derived either from gas producers or from coke ovens, which in combined plants are always available. Even with steam driven blowing engines, such as are employed now almost exclusively in American practice, it has been commended to provide for some means beside blast furnace gas for supplying additional power or blast at need.

9 The best equipment which can be provided for such cases of emergency consists of a bank of gas producers which can deliver a regulable quantity of rich gas to the boiler plant where it can be burnt together with the blast furnace gas under perfect control, instead of using additional boiler coal. Where these wise precautions are taken, the breaking down of one furnace would initiate no appreciable amount of trouble and no interruption in the power service, since the rich producer gas can be made to fully replace the loss of poor blast furnace gas. It is known that producers, when properly constructed and subdivided, can be overloaded by 100 per cent and more, if the necessity should arise, also that they can be put in operation from cold within a very few minutes, and that they can be fired with inferior grades of coal, mine culm, etc., of which there are enormous quantities available in every large plant.

10 Since of the three available sources of gas generation; the blast furnace, the coke oven, and the producer, none is worked at its utmost capacity under normal conditions, it is quite easy to provide for the required gas energy from the combined equipment, and without hav-

ing any special reserves. Where blast furnaces are the only source of power, there the provision of spare producers is commendable both for securing greater regularity of product and for having sufficient reserve for use in the central station.

11 The electric canalization of industrial districts and the system of power exchange which is now so widely adopted in large scale operation in Europe, will tend to further secure stability of production independent of local breakdowns.

12 Mr. C. C. Atwater. It should be borne in mind that my subject is substantiated by such data only as were obtained in European practice. This explains the difference in the quantity of gas output between American and European coals. Dr. Hoffmann-Bochum is the authority quoted regarding the figures of performance of Otto ovens.

13 I will modify my statement that the gasification of bituminous caking coal is not an accomplished fact, to the effect that it is not a commercial proposition. I know of several producers in this country which were originally working with such material, but are now operating on anthracite instead, because the operative difficulties in actual practice proved to be insurmountable. For further information reference may be had to an article on "The Application of Gas Power in Collieries" which I am publishing in the "Engineering and Mining Journal."

A PLAN TO PROVIDE FOR A SUPPLY OF SKILLED WORKMEN

By M. W. Alexander, Published in November Supplement

Mr. John Knickerbacker The paper on "A Plan to Provide for a Supply of Skilled Workmen" is one that could be profitably read by every employer of skilled labor in the country, as the employers might find some way to introduce in their work a plan that would eventually add to the efficiency of their force and give the boys a better chance.

2 There is a scarcity of good workmen and a lack of general interest on the part of employers to encourage boys to learn mechanical trades. To overcome these conditions it is necessary to first arouse the interest of the employer in the training of young men, and the interest of the young men will follow, for the desire for advancement is not yet killed by the preacher of class hatred. There is a moral and patriotic duty to train the young men in our employ, but this

cannot be accomplished without some well directed comprehensive method, and methods will be found if we try to discover them.

3 It seems to me that so far as it lies in the power of this Society, it should endeavor to create a strong interest in this subject of training the apprentices by encouraging the submitting each year of papers on this question. This is a matter that is receiving the attention of some of the large manufacturers' associations, notably the National Founders Association. They have within a year encouraged the establishing of a model foundry school at Indianapolis under the charge of the Winona Assembly, and it is the desire of many of the members of that Association that that school shall become a model for the foundries of the country to follow in the training of their apprentices.

4 It is not an easy task to train these young men, and in order to obtain satisfactory results it is necessary to make careful selections, and what has been done by the General Electric Company shows that this process of elimination has been carried out, for of the 462 boys at one time or another in the course at Lynn, 171 are still on the apprentice shop course, 37 are serving the trial period, and 29 have graduated. These 237 men and boys are about 51 per cent of the total number of boys received. It is to be earnestly hoped that the generous example of the General Electric Company will be followed, and that this Society will do its utmost to help along the good work.

PROFESSOR WILLISTON There are several points which I would like to emphasize in this most admirable paper of Mr. Alexander, in which he describes what is being done in the Lynn works of the General Electric Company to train and educate for efficient service in later years the young men and boys who are entering their employment.

2 I will not enlarge upon the need that exists at the present time for skilled workmen or the still greater need for intelligent and competent foremen and assistant superintendents to plan and direct their work; nor will I dwell upon the fact that the progress which thus far, has been made in this direction, here and there where modern systems of supervised apprenticeship have been introduced, is so slight as to amount to practically nothing when compared with the immensity of the whole problem of providing trained and competent workmen for the present and future requirements of the rapidly growing mechanical industries of this country. These facts are too well understood and too self-evident to need either demonstration or argument before this Society.

3 The first point, however, in Mr. Alexander's paper to which I wish to call attention, is his inclination to hold the manufacturers themselves largely responsible for the small amount of progress that has been made in this direction. He blames them, and I think justly, for not having found more effective means of joining forces with the educators. In this, the initiative properly belongs with them. They are the only ones who know with accuracy the requirements, or the type of individuals who will be likely to succeed in their work. Without their aid and coöperation the professional educator will be able to make but little headway. They, too, are the ones who have the greatest financial interest in the problem. In many instances the future success or failure of their business may depend on its proper solution, and a large part, or even the whole, of their invested capital may be at stake. Buildings and the best equipment are worth but little when not manned by an organization that can cause them to make money.

4 President Eliot said of Harvard University, when questioned a few years ago regarding the cost of duplicating it in a western state, "The bricks and mortar probably could be duplicated for about ten million dollars, but two hundred and fifty years of experience come high," and similarly one of our past Presidents has said in comment, after passing through one of the large and recently established industrial plants visited a short time ago by the Society, "*Money will purchase a magnificent equipment, but it takes years to perfect an organization.*" I repeat these remarks in order to emphasize the fact, as Mr. Alexander suggests in his paper, that an efficient body of trained subordinates is the most important asset in many a prosperous business; and also to emphasize the length of time which it takes to build up such an organization if it is low, or if it has been allowed to run down.

5 If these facts were realized in their full significance, I believe that the manufacturers of this country would feel that it was worth while for them to coöperate in some effective way for their own future interests, and would appreciate—in the popular parlance of the day—that it was "squarely up to them" to take some effective steps to insure a sufficient supply of high grade subordinates for their future need.

6 The second point that I wish to refer to is, that Mr. Alexander's plan aims to provide men unlike those produced under the older apprenticeship system with the fixed ideas and equally fixed prejudices, but instead, a new type of skilled mechanics who will be progressive, resourceful, and, so far as possible, ambitious to coöperate

in the development of the company. His hope is to train men of intelligence, who can reason from cause to effect, who can perform one process with some relation to the process which is to follow; who can readily adapt themselves to new conditions, and who are receptive and eager for new ideas: men who believe, not with the old line mechanic, that their method is always the best, but who have faith that always there is a better method possible that is worth their striving for, which they can reach if they are open minded and have patience to persevere. This is the type of man that large organizations must rely upon for their future strength.

7 Those who have received as their early training—as is usually the case in the regular apprenticeship—nothing but a chance to imitate the handwork and the methods of others will not, except in very rare instances, be fitted to rise into the many important subordinate positions found in a modern manufacturing plant.

8 The means which Mr. Alexander has adopted by which he hopes to accomplish these results are worth the most careful consideration. He frankly acknowledges that neither the old method of apprenticeship nor anything that approaches it will produce the desired result. The special foreman whose duty it is to look after the interests of the apprentices may be of some service, but they alone cannot accomplish what is wanted. Much more is required.

9 In the system in vogue in Lynn, they have practically turned all the old ideas regarding the training of apprentices completely upside down. Instead of giving to the boys, as is usually the case, any old thing to keep them busy, making them run errands and do all sorts of monotonous and uninteresting tasks which require neither intelligence nor skill, they have given them the best that their shops afford.

10 They have given them the best and most intelligent foremen in the factory to act as instructors, and have chosen for the type of work which they should do such things as the rebuilding of old tools, so as to insure higher degrees of accuracy; the making of jigs, the special tools and fixtures, tool making, and even practical tool designing. In other words, they have chosen for them the best work and the best conditions in their entire works.

11 These things are evidently wisely planned, with the idea of cultivating intelligence and reasoning power; of broadening the view of methods of manufacture, and lastly, but most important of all, of stimulating ambition.

12 It is not necessary for me to compare the influence of this treatment with that of the experience which surrounds many and many

an apprentice boy in an average modern manufacturing plant, where he encounters rebuff after rebuff which tend only to dampen his enthusiasm and crush his ambition. We cannot wonder that the boy who starts on his life work, under such conditons, at fifteen or sixteen years of age, full of hope and determination, finds by the time he is but little past twenty, at the end of the period when he should have been growing and developing most rapidly that—unless indeed he has remarkable courage and pluck—he has less ambition, less receptivity for new ideas, and less willingness to coöperate for the general good of his company, than at the time he first entered their employ.

13 There are other points in this paper which should be taken up and discussed. Some of these are: the desirability of having the man who knows most about the boy's intellectual attainment determine his pay; the value of the certificate given; the effect of such a system on the attitude of all the employees toward their work and toward the company; the possibilities of the evening courses that Mr. Alexander has suggested, and the possible coöperation between the public schools and school shop.

14 To summarize the paper, let me say that it is a most practical demonstration of the fact that, as a cold business proposition, it pays for a company to go almost to any expense in order to do everything in its power to stimulate the ambition of the boys it takes into its employ; and that any money spent to help them to believe more thoroughly in themselves, to have more confidence in their own intelligence and powers, and to have more faith in their capacity for future development will, like the bread that was cast upon the waters, be returned after many days. We have heard things like this before without the figures to substantiate the statements. It is therefore most gratifying to know that in this instance the General Electric Company finds, as a matter of actual bookkeeping, that the balance here is all on the profit side of the ledger.

15 In conclusion, I wish to thank Mr. Alexander for bringing this matter before the Society and to congratulate him on the splendid work that is being done under his direction at Lynn.

MR. M. W. ALEXANDER The discussion, in which several gentlemen have kindly participated, has brought out many interesting points that are worthy of consideration, and has indicated that my plan of training young men for all-around skilled artisans has not been quite fully understood as relating to the methods employed in the apprentice department at the Lynn works of the General Electric Company.

2 It is claimed that separation of the apprentices from the journeymen will produce disadvantageous conditions for the boys. This segregation, however, as I have explained, takes place only during the first two years of the apprentice course, when the boys are grouped in a training room where they receive systematic instruction in the trade on commercial work, the remaining two years being spent among the journeymen in the different departments of the factory.

3 This plan is justified by the consideration that, inasmuch as the attitude of many of our workmen of today toward work is not conducive to the best interests of the boys, apprentices should not be surrounded during the initiatory period by workmen, but should be kept under the sole influence of two or three picked instructors. These instructors endeavor to teach knowledge and skill as well as to build up the character of the boys, instilling into them honesty of purpose and a sense of their obligation to perform their work with the greatest speed and accuracy, regardless of wages or any other similar consideration.

4 When such attitude toward work has been sufficiently developed in the apprentices as will fortify them against adverse influence in the shop, they are then assigned to different factory departments where they work side by side and in competition with the journeymen for the remaining years of the apprentice course.

5 The training room, however, has another function to fulfill which is of great importance and which has evidently been misunderstood by one of the speakers in the discussion. It is true that only three instructors are engaged in teaching skill to about one hundred apprentices in the training room; this being made possible by utilizing most of the apprentices themselves for assistant instructors. It must be borne in mind that the regular teachers instruct all boys, but after an apprentice has once been shown by the instructor how to perform an operation, he can safely be put under the guidance of an older apprentice for further practice of the particular operation, with the regular teacher looking after him and his boy instructor as the occasion requires. Thus the apprentice training room is run economically and the apprentices are taught to teach. No further argument is needed to demonstrate the efficacy of this plan than the fact that it has proved eminently successful during the five years of application at the Lynn works of the General Electric Company.

6 The functional foremen system, which has been advocated in the discussion, will not bring about these results; its chief value lies in the possibility of turning unskilled workmen into skilled specialists. The same end may be reached through a course of prac-

tical evening instruction, such as I have outlined in the latter part of my paper.

7 It has been pointed out properly that the wages paid to our apprentices are considerably higher than those usually paid in other factories. I cannot, however, agree with the plea which has been made for a still further increase of 50 or 100 per cent in the wage schedule in order to attract to the apprentice course young men of from 20 to 25 years of age rather than boys of 16. While we do not close the door of apprenticeship to desirable young men of mature age, I doubt that, except in individual cases, they will prove of greater worth both during the apprenticeship and afterwards, than the boys of 16 who come fresh from the school. Having usually spent their time between the ages of 16 and 20 in aimless work, these young men are not generally amenable to systematic instruction; often they have gotten into the rut of daily routine of unskilled work and are not sufficiently ambitious to pull out of it. Had they been taken in hand at the age of 16 they would probably have entered a field of skill at that period and avoided the forming of bad habits which have accumulated during the years of floundering and must be corrected during the years of apprenticeship.

8 In the long run, he will prove to be a more efficient and desirable artisan who has been under systematic training from his sixteenth year, or from about the time when he left school, than he who begins to acquire skill at a later period of life. Furthermore, is it not our duty to take care of the boy of 16 or even 15, if he is obliged to leave school at that age, and start him on the right road? The healthy boy is able to engage in mechanical work even at that age, especially if the work is selected judicially. The contact with real industrial life will, furthermore, mature him much quicker than would be the case during the same period of time spent in the school.

9 After the apprentices have been at work for about six months, and not any sooner, are they obliged to attend classroom exercises. The purpose of this academic instruction, however, is not intended chiefly for the purpose of giving the boys a general education that they may not have received in the public schools; it is rather intended to correlate the practical work of the shop and the theory that underlies the work, in order to illumine the former by the latter, so that the boy may be able to apply in the work-shop, day by day, the knowledge which he gains in the classroom. Incidentally the classroom instruction endeavors to rectify some of the defects which are found in the educational equipment of the boys.

10 I admit that most of the classroom instruction and the early

part of the practical training may be given to advantage in special industrial schools established as a part of the public school system, so that graduates from such schools may enter upon an advanced apprenticeship in the factory. Until such schools are established, manufacturers must take the responsibility of training their own recruits. The efforts of some manufacturers' associations in founding practical training courses for boys who desire to learn a trade must be heartily welcomed; private enterprise has usually provided the initiative which precedes marked advances in the public school system.

11 The problem of retaining the apprentices after graduation has been pertinently pointed out by several gentlemen. A reference to the statistics which I have given in my paper will show that, as far as our apprentices are concerned, more than 80 per cent of the graduates have remained in our employ. The problem, therefore, does not exist for us at present, although I realize fully that we may not always be able to maintain as large a percentage; in fact, our expectations right from the beginning have been that we may be able to retain $\frac{1}{3}$ of the graduated apprentices. After all, we must take a broad view of the question and content ourselves with having improved the personnel of American factories in general, if we cannot confine the improvement to our factory alone. In some instances the young journeymen will undoubtedly be benefited by what the Germans call "Die Wanderjahre," where the contact with new conditions will round them out and broaden their horizon. The chances are that many of those who have left the company will return to their alma mater better men for themselves and for the company.

12 We encourage all desirable graduates to remain in our factory by offering them good wages and good prospects. Depending upon the individual worth, we pay graduated apprentices from \$2.50 to \$3.00 per day, and advance them as the excellency of their work warrants. Several of the graduates have already been placed in positions of responsibility, two serving as assistant foremen. All apprentices know that it is our hope and intention to have most of our future foremen and assistant foremen recruited from the graduates. A strict and almost severe training during the four years of learning and a liberal policy toward those who have successfully finished their course will, in my estimation, prove to be the best policy.

13 As our industries develop more and more, the position of the industrial foreman will become more and more important. His equipment must be skill and knowledge; how great a part of the allotted time is to be devoted to the one or the other will depend very much on the educational preparation of the boy and on the nature of

the trade that he wishes to learn. Whether the apprenticeship course is a "half-time school," or as I prefer to call it a "part-time school," matters not as long as skill and knowledge are taught in a correlated manner.

14 I am sure that the system in vogue at Lynn will undergo further developments; the key note, however, which we struck right at the beginning and to which we shall hold fast, is to stimulate the ambition of the apprentices at all times. It is for this reason that we have set no fixed schedule for the time allotted to the various operations in the practical work; each boy is required to perform the operation in an entirely satisfactory manner both as regards speed and accuracy, and as soon as he has accomplished these results he is allowed to proceed to the next operation. It is interesting to see at times a boy instructor of but six months' of service guiding an apprentice who has been on the course for nine months and more. Too often, as has been pointedly remarked by a gentleman, the ambition of the boy is stifled rather than stimulated under other forms of apprenticeship.

15 In conclusion, let me urge upon all manufacturers the necessity of giving serious thought and of applying a liberal policy to the training of the future skilled workers in their factories. Let them coöperate with the educators in an endeavor to make the educational system meet the industrial demands, but, in the meantime, let them not rest on their oars, but supplement their educational advice by the demonstration of educational activity.

THE FLOW OF FLUIDS IN A VENTURI TUBE

By E. P. Coleman, Published in November Proceedings

Professor Williston There is one point regarding the Venturi tube which, thus far, has not been brought out in the discussion: that is, the difficulty of measuring with sufficient accuracy both large volumes and relatively small volumes of air flowing at different times through the same meter. In some experiments which I made in St. Louis a few years ago, this was forced on my attention. I had occasion to measure the flow of air, and used three different methods of measuring it; first, carefully calibrated anemometers; second, circular orifices of various diameters; and third, the Venturi tube. These three methods were used partly to get the best results under different conditions of flow, and partly as a check on one another. As a rule the result from all agreed closely.

2 When the velocity of flow through the Venturi meter was approximately that for which it was designed, the results obtained

were found to check with the results of other methods within less than one per cent, but when I attempted to measure small quantities of air with the same meter I found that the differences in pressure due to the small velocities obtained was extremely small. The fluctuations in pressure due to eddies and pulsations of air made the use of a very sensitive differential gage impractical; and without this the accurate determination of the flow was almost impossible.

3 With the circular orifices and with the anemometer this difficulty was not encountered, because it was such an extremely simple matter to change the size of the orifices and thus keep the velocity of flow approximately at any desired point at which the difference in pressure could be easily and accurately measured. The same thing, of course, could have been accomplished by using several Venturi tubes, but it would have required a number of meters of different sizes which could be readily substituted for one another as the volumes of the air to be determined changed. In many cases this would be impracticable, while with orifices different sizes can be substituted for one another in a slide properly prepared to receive them, at a moment's notice, and at slight trouble and expense. When the quantities of air, however, to be measured come within the range of a single meter I have found the Venturi tube most satisfactory, and I feel that we are indebted to Mr. Coleman for bringing this subject to the attention of the Society.

MR. E. P. COLEMAN With regard to a point brought out by Mr. Herschel, the instrument calibrated was in reality a converging nozzle, the down stream cone of the meter having been removed for greater convenience when inserting the Pitot tube into the throat. The formula, however, being based on constant energy per unit of fluid mass for steady flow in any pipe, is independent of all portions of the pipe, with the exception of the two sections considered, viz., upstream and throat. It would seem, therefore, that the absence of the down stream cone should not affect the coefficient of discharge at the velocities obtained in the experiments. The Venturi meter and the Pitot tube are rate meters, the Pitot tube presenting the greater opportunities for personal and other errors. The Pitot tube was used because of its well established coefficient, and trifling cost.

2 Replying to the question of Mr. Moss as to the coefficients of discharge, I wish to say that a coefficient of unity was used in all of the calculations. This gave, when measuring the air, two quantities in pounds per second for each experiment; *i.e.*, Q as calculated from Venturi observations, and W as calculated from Pitot tube observations. The average ratio of Q to W, as shown in the experiments

with air, was 0.995. Hence, as far as these experiments show, if the coefficient of the Pitot tube is unity, the Venturi coefficient is 1.005; whereas, if the coefficient of the Venturi meter is unity, the Pitot coefficient is 0.995. This coefficient I believe to be constant for throat velocities of say 50 to 500 feet per second, and therein I consider the superiority of the Venturi meter over anemometers, standard orifices, etc., in which the coefficient of dischage is a function of the velocity, and must be determined by calibration.

A MECHANICAL ENGINEERING INDEX

By Prof. W. W. Bird and Prof. A. L. Smith, Published in November Proceedings

Profs. Bird and Smith Professor Breckenridge in his discussion of our paper says that alphabetical indexing should be a thing of the past, except for indexes of a limited scope. In reply to him, we would say that our index is of a limited scope, inasmuch as no subject is introduced unless it is connected in some way with Mechanical Engineering. On the other hand, however, its use is not limited to those possessing a key to the Dewey System. Anyone who can spell can readily find a given topic, and it has been our experience that a stranger has no difficulty whatever in using our index. Therefore, the main object has been accomplished—we have a system which requires neither an attendant nor a book of explanations.

2 Our list of titles and sub-titles is similar to the Relative Index of the Dewey System, which is also arranged in alphabetical order and differs from ours in that a number has been assigned to each title. This system of numbering is of great value in the matter of filing books and catalogues and we have adopted such a system for our catalogue file. We did not consider it necessary to use numbers on our book references, as we maintain our Department Library simply as a working library of the latest books and best authorities. All our books are numbered according to the Dewey System and can be located in the usual way by reference to the regular Library Index.

3 No system can produce a grouping which will put all the material on a given subject in one place, for the reason that all subjects are inter-related like the meshes of a net and not like the branches of a tree. And we agree with Mr. Hess that if we are to have a universal system it must be an arbitrary one. We should be glad to see some such scheme as he has suggested adopted by all engineering schools and societies, and we believe that it would be of the greatest benefit to the profession. In the meantime, we can all help in the good work if we

will see that all catalogues for which we are in any way responsible are of the standard sizes, and that all books and contributed articles have logical titles, *i.e*, titles which have a definite relation to the subject matter.

4 Mr. Clemens thinks that our system does not lend itself readily to expansion. If he will stop to consider that it is a card index, and that there is a place ready for any new topic from *A* to *Z*, he will see that his criticsim applies only to our list of subjects as published, which of course is fixed for the present at least.

5 It was our hope and expectation that this list would be discussed and that we should have the privilege of adopting numerous valuable suggestions so that in its final form the list of subjects would be the result of many minds.

6 In closing, we would say that this system of indexing is a part of our department organization, and we have brought it to the attention of the members of the Society not because we think that it is the only way of indexing, but rather to let others have the benefit of our experience along this line, which we believe has resulted in a successful system.

PRODUCER GAS POWER PLANT

By J. R. Bibbins, Published in November Proceedings

Mr. J. R. Bibbins Reference was made in the foregoing paper to the effect that more recent data indicated considerable improvement in the operating efficiency of the Gould plant over that first reported. This is due somewhat to more careful operation but largely to the increased loading of the plant. The month of June, 1906, may be taken as a typical example of these latter results:

Length of run (daily including Sundays)				24 hou s.
Daily output	max. 9700	min. 1600	avg.	7500 kw.h.
Station loading factor	max. 90%	min. 15%	avg.	69.5%
Net coal per kw.h.....	max. 1.97	min. 1.53	avg.	1.78 lbs.
Extra fuel for building new fires (6%+)				0.108 lbs.
Total fuel per kw.h.				1.88 lbs.
Plant efficiency (running) ..	max.16.6	min. 12.9%	avg.	14.5%
Plant efficiency including extra fuel for new fires.....				13.4%

2 The accompanying curves summarize this data as well as that presented in the foregoing paper. Observations now cover a range of loading from 25 to 90 per cent of the station capacity. A noticeable effect of the higher loading is a departure from the straight line referred to in the paper as representing the relation between total

coal consumed and output. There is a noticeable bend in this total coal line indicating a higher efficiency of working on the higher loadings. This point would not be apparent from the fuel rate curve. It results in a constantly increasing plant efficiency. It should be noted that these curves represent averages as determined by the graphical method previously described. The results are as follows:

Station loading factor	50%	75%	100%
Coal per kw.h.	2.1	1.85	1.68 lbs.
Plant efficiency	11.7	13.6	15.2%

All standby losses as well as mechanical and electrical losses are here taken into account, so that the gross efficiency of conversion of coal into measured electricity is represented by the figures given, 15 per cent at full load.

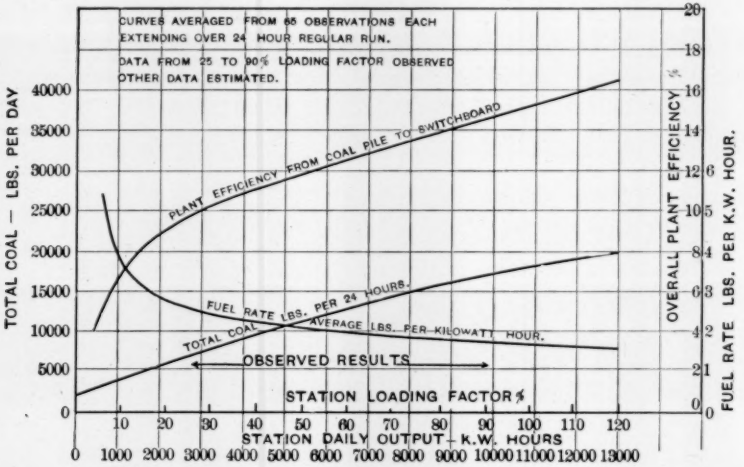

CURVES AVERAGED FROM 65 OBSERVATIONS EACH EXTENDING OVER 24 HOUR REGULAR RUN.
DATA FROM 25 TO 90% LOADING FACTOR OBSERVED OTHER DATA ESTIMATED.
PLANT EFFICIENCY FROM COAL PILE TO SWITCHBOARD
FUEL RATE LBS. PER 24 HOURS
TOTAL COAL — AVERAGE LBS. PER KILOWATT HOUR.
OBSERVED RESULTS
TOTAL COAL — LBS. PER DAY
OVERALL PLANT EFFICIENCY %
FUEL RATE LBS. PER K.W. HOUR.
STATION LOADING FACTOR %
STATION DAILY OUTPUT — K.W. HOURS

FIG. 1 SUMMARY OF OPERATING RESULTS, GOULD COUPLER CO., DEPEW, N. Y. PRODUCER GAS PLANT

3 In the original paper the cost item of repairs could not be definitely given, and it was necessary to estimate this quantity owing to the absence of separate data. This estimate checks closely with later records for 1906 which, however, show a rather high labor cost. This is due to the fact that a certain number of men are required to operate a plant of this character regardless of its capacity up to a certain point. In this particular instance it is safe to say that the same force could readily handle double the plant capacity. Assuming the station loading factor to be about 70 per cent, a distribution of cost items is approximately as follows:

	PER CENT TOTAL COST	PER CENT OPERATING COST
Fuel	27.4	38.4
Wages	29.5	41.4
Supplies	5.8	8.2
Repairs	8.6	12.0
Operating	71.2	100.0
Fixed	28.8	
Total cost	100.0 = 93c. per kw.h. = $60 per e.h.p. a year	

4 The apparent discrepancy which Mr. Ennis has observed in the relative floor space of the Gould power plant is due to his misinterpretation of the data in Table 1. In one case the area of the power house is given: in the other, that of the two compartments of the cooling pond outside. The relative floor space of the former is as stated—6.2 feet per kilowatt.

5 The term "loading factor" may very possibly be misleading, but was used in the absence of a standardized expression. "Load factor" in its true sense is definitely restricted by long usage to a central station load which is continuous and practically devoid of violent fluctuations. There is, therefore, little chance for misapplication. But with the fluctuation load of an industrial plant, such as that under description, the term is decidedly indefinite with frequent swings, 50 per cent or more above the average carried at the time, it is very doubtful if the quantity

$$\frac{\text{Average load}}{\text{(Instantaneous) Maximum}}$$

would represent load factor as accurately as

$$\frac{\text{Average load}}{\text{(Steady) Maximum}}$$

The latter, however, is somewhat of an anomaly.

6 It would seem to the author that a clear distinction is necessary between terms relating to the outside business or connected load of a central station and those concerned only in the relative load of individual units or the station as a whole. The former is external to the power plant, the latter internal, and most closely involving operating efficiency, and as rated generating capacity is the usual reference point in expressions of efficiency the term "loading factor" was chosen as the most appropriate: *i.e.*,

$$\text{Station loading factor} = \frac{\text{Average station load}}{\text{Total rated capacity}}$$

Applied to an individual generating unit, this is equivalent to the "per cent full load rating," which may readily be found as the ratio of the integrated areas of load curve and engine capacity curve respectively. As the terms "loading" and "capacity" are so dissimilar in meaning, the author still considers the former preferable. "Capacity factor" would seem more applicable as an expression indicating the excess generating capacity available above the maximum load carried, *i.e.*, what Mr. Ennis designates by still another term "peak factor," or the ratio of maximum load to rated capacity.

7 The most useful expression from the standpoint of operating efficiency is the ratio

$$\text{Loading factor} = \frac{\text{Average running load}}{\text{Rated capacity in operation}}$$

which is the true "loading factor" for any length of run of whatever duration. Excess plant capacity does not effect the validity of this term, but it is taken into account in "station loading factor." The former applies with equal accuracy to a 10 hour or 24 hour continuous load; but the latter covers losses of standby as well as running periods. In the former we have an exact check of the proportioning of plant capacity to load; in the latter, a measure of the duty imposed upon the equipment, while the original term "load factor" indicates the possibility of increasing the output and revenue during lightly loaded periods of the day.

8 In connection with this subject of power costs, the author cannot resist the opportunity for earnestly recommending the keeping of daily records, in which an astonishing number of plants of very respectable size are at fault. In no manner can irregular and faulty operation be more readily detected than by reference to the fuel scales, water meter, and watt meter on the daily log. Very little time is required to make these simple observations, and in the making much is learned by a wide-awake operator, who takes proportionately greater interest and pride in the plant under his charge. Few men are insensible to opportunities for self improvement and here is an opportunity that has been much neglected in the past and is even today. The mere taking of meter readings is not the essential point but rather the interpretation of them by the engineer himself.

9 From this neglect arises the difficulty of obtaining economic data often desired for comparative purposes. Every power plant manager seems to use a different system or basis of calculations with

the inevitable results that data of this character are very generally misleading unless one is completely familiar with the surrounding circumstances. It is the author's belief that much good might be done in the formulation, by some engineering body, of a definite scheme or code for the determination of power costs upon a broadly comparable basis somewhat similar to that in force in Great Britian by legislative enactment. As fuel becomes dearer in America this matter will receive proportionately greater and greater attention.

FERROINCLAVE ROOF CONSTRUCTION

By Alexander E. Brown, Published in October Proceedings

Mr. A. E. Brown Some of the statements in the discussion of my paper by Mr. Eugene N. Hunting seem to call for correction as they are evidently based on a lack of knowledge of what Ferroinclave actually is and does, and has done in practice, in the accomplishment of the object for which it was designed.

2 In the first place, I wish to correct the impression he evidently has and to say that Ferroinclave and Ferrolithic are not one and the same thing, neither are they constructed alike, nor act exactly alike when used in roof construction. He couples the two together in various parts of his discussion as though they were one and the same thing, and speaks of it as "Ferroinclave" or "Ferrolithic."

3 I believe my paper clearly shows that Ferroinclave is a true dovetail corrugation, having a well determined and proportioned key or lock in its form, which by reason of its width and depth, and the underlocking of the dovetail is best suited for holding the cement. The underlocking is about $\frac{1}{8}$ inch on each side, or nearly $\frac{1}{4}$ inch at the bottom of the corrugation wider than that at the top.

4 The most characteristic and important factor in Ferroinclave is the fact that the adjacent corrugations are tapered in inverse directions; that is, in a 10 foot sheet, the corrugation at one end is $\frac{3}{16}$ inch wider than at the other end, and the adjacent corrugation is the reverse.

5 In Mr. Hunting's discussion, under paragraph 7-a, he makes the statement that Ferroinclave is not a true reinforcement for the concrete, and that the entire metal section lies very close to the neutral axis.

6 This is evidently a mistake on his part, for there can be no more complete reinforcement than a continuous sheet of metal arranged to give a solid and perfect lock, or bond, in concrete cement. The outside dovetail action is continuous and solid, whereas any locking, by

means of perforations only, depends on sharp edges and small portions to anchor in the concrete. It is absolutely impossible for the concrete to come out of these corrugations without being literally torn to pieces.

7 If Mr. Hunting had noticed the various tests which were made in actual practice of various sheets and thicknesses of concrete, he would have seen than when carried to breaking, the sheets invariably tore in two by mere tension, without the slightest effect of sliding of the concrete along the sheet. This has been proved by a great number of tests under varying conditions.

8 Besides the adhesion, which can be secured by the metal itself, the very form of the dovetail corrugation in its lengthwise direction, also prevents horizontal shear, by reason of the tapers of the corrugations in two directions; the material wedging as well as adhering. In other words, the wedging action prevents horizontal shear, even before breaking of adhesion could occur. Besides, the continuous adhesion throughout the entire length of the sheet makes a far more perfect bond than intermediate notches or open spaces, where the accumulation, through the sheet itself, of small increments of adhesion are always applied in the concrete at the point of breaking.

9 The other part of paragraph 7-a, in which it is stated as an objection that the entire metal section lies very close to the neutral axis, is also a mistake, and would not have been made if Ferroinclave had been carefully examined.

10 In my paper I have stated that the depth of the corrugations in all cases is slightly under $\frac{1}{2}$ inch, and that only enough cement on the underside is put on to cover the lower surface of metal up to $\frac{3}{8}$ inch; this is done even in floor construction, where the total cement slab is as much as five inches thick.

11 The central line of the Ferroinclave when in place, therefore, is never more than $\frac{5}{8}$ inch from the bottom of the lowest portion of the plaster. The very form of the dovetail corrugations gets the maximum amount of metal possible near the bottom to form the bottom chord of the concrete steel beam of which it is a part.

12 Samples of Ferrolithic sheets that I have seen and examined thoroughly all have parallel corrugations, and the dovetailing or key effect is so small, or the interior angle so small, and the spaces so wide, that it is probable there would be great difficulty in securing a perfect bond or lock of the cement, particularly on the underside; whereas, the interior key and the horizontal taper of the Ferroinclave is so decided, and the spaces so proportioned, that it is nearly a theoretical proper proportion for locking the plaster. In the same paragraph "*c*" is I

suppose only a supposition without experience on the part of the writer, as a large number of fire tests have been made with the Ferroinclave in about 5 feet spans, the same as is ordinarily used in roof construction, where the span has been loaded at the same time to more than double its ordinary working loads; the whole sheet and mass being heated to a white heat, and left to stand for some time, when water was thrown on it with full pressure through the hose, and under these conditions loads were dropped on the already loaded beam. After this test there were found to be no signs whatever of the cracking of the cement away from the sheet, the only effect being slight hair cracks in the cement itself on the under surface. The cement was not in any way broken loose from the metal. In other words, it is impossible for the cement under these conditions, to break loose from the dovetail as proportioned in Ferroinclave.

13 There seems to be an impression, also in the discussion of this article, that in the heating and cooling of Ferroinclave, with its cement and sand covering there would be a great difference in expansion between the metal and the cement itself. On the contrary, the coefficient of expansion of good Portland cement and steel are almost absolutely the same, and the mixture of two parts of sand to one of good Portland cement is so nearly the same that under the conditions described they act together.

14 I am pleased to answer Mr. B. N. Bump's inquiry as to the permanency of Ferroinclave as used in roof or floor construction. When two parts of sand and one of good Portland cement are properly applied to the Ferroinclave sheets, the sheet itself will last indefinitely; the only chance of corrosion of the sheets in any way is by accidental cracks, and then corrosion will occur only at or near the cracks.

15 This has been determined by many trials and examinations of roofs that have been in use for nearly five years; and in fact, we have made several exceedingly severe tests for determining this very thing; one of them, particularly, where the cemented sheets were subjected to nearly a flood of water all the time, and in another case where the cemented sheets were in water all the time for over a year and a half, and when the cement was cut off, the iron was as clean and free from rust as when first rolled.

16 To avoid the possibility of any bad effects or cracks, due to improper roof construction after the Ferroinclave and cement has been erected a waterproof covering of some form is applied, which is elastic enough to span over any small cracks that are due to motion that may be in the imperfect construction of the roof of the building.

17 With three or four roofs that were put on when Ferroinclave

was first designed, we used gypsum plaster on the under side instead of cement. The sulphur which the gypsum contained while it was still damp, caused chemical action upon the metal and sometime after, by the porosity of the gypsum which absorbed further moisture, corroded the sheets more or less. By the swelling of the oxide products from this chemical action, some of the plaster was found to come off where exposure to dampness on the underside was the greatest. This occured on the first roof where Ferroinclave was used, but since that time the proportion of two of sand and one of cement has been adhered to, and there has been no such action.

18 In concrete steel construction generally, it is very certain what will be the condition of the steel used for reinforcing after a number of years; that is, there will certainly be two results only:

19 In constructions where the proper proportion and the proper quality of sand and cement are used, honestly and properly applied, there will be no deterioration of the steel for generations to come.

20 In constructions where ashes, and porous cinders, rubbish, and poor cement are substituted, and indifferently and improperly applied, the steel will deteriorate until the very structure is destroyed in the process of corrosion.

21 In other words, there is no form of construction in engineering requiring more skill and care from the designing to the final erection, and requiring more honest and painstaking watchfulness of details from start to finish, than reinforced concrete construction, and it is certainly a dangerous construction unless in the hands of competent and honest engineers and contractors.

SAW-TOOTH SKYLIGHT IN FACTORY ROOF CONSTRUCTION

By Mr. Fred. S. Hinds, Published in October Proceedings

Mr. F. S. Hinds If Mr. Henderson's discussion would apply to trusses on a span of 35 feet I would like to reply in connection with the point I have endeavored to emphasize and which also answers other discussion, that 24 inches to 48 inches in width between each skylight cannot consistently be called a gutter, but is really a flat roof. In designing a new shop, even in the western states, I could introduce trusses four to five feet in depth and carry out the features which I have demonstrated. In fact, on an addition to the Mohawk Carpet Mills at Amsterdam, N. Y., I designed last year a saw-tooth skylight roof for the steaming and coloring departments where I introduced wood trusses in the middle of the flat space between

each skylight. That is, taking the place of the girder and form above same as shown in Fig. 3, and *starting the top chord* of truss at the roof plank. The span was only a matter of 28 feet, and for this I used wood rather than steel.

2 I would like to answer Professor Sweet's discussion, for the reason that he claims I made some mistake. Now, the first mistake that he makes in his own discussion is the statement that I claim to be the first to apply it to a machine shop. In my paper I refer to the use of it in lighting the area in a mill yard or textile mill and not a machine shop. Therefore, another man deserves the credit of first introducing this type of skylight on machine shops, and evidently from what he states it is very satisfactory.

3 In discussing the other features, I will say that Professor Sweet's method, while a good one, would be more expensive than the other, and secondly, it does not provide for the matter of condensation, which in cast iron gutters and below the roof line would, with the low temperature outside, tend to condense even greater than if the flat space between the skylights was a part of the roof itself.

4 In the hanging of shafting to this form of roof construction, it is applied to a great variety of manufacturing plants—in some cases for lighting a room that has no shafting; in other cases where the skylight is presented to the north and across the shop or factory the shafting could be very readily supported, and has been done in a number of cases, by the simple method of using two wooden girders, or steel frames spanning the skylights at any point where the shafting lines cross them.

IMPROVED TRANSMISSION DYNAMOMETER

By Prof. W. F. Durand, Published in November Proceedings

Prof. W. F. Durand Regarding the comment on the paper, no closure is required other perhaps than reference to the remarks by Professor Flather regarding ball bearings. I quite agree that for larger capacities a roller bearing would be more durable, and in the larger design, to which reference is made, a special form of double "high duty" ball bearing is employed. It may be said, however, regarding the smaller pattern described in the paper that no trouble has arisen in connection with the bearings except when run at an excessive overload (about 100 per cent) when one or two presumably defective balls were broken, with some consequent cutting of the bearings.

TEST OF A ROTARY PUMP

By Prof. W. B. Gregory, Published in November Supplement

Prof. Gregory The writer wishes to state that he had no preconceived theories to prove and no other object in view, in making the tests described in the paper, but to ascertain the truth. Results are not necessarily wrong because they are better than was expected. Below are given a few reasons for believing the results stated are accurate within a reasonable limit of error.

2 The Abbeville plant was tested during the summer of 1905; it was the first of many plants tested that season. The indicators were new; they had never been used before. They were carefully calibrated and behaved in a satisfactory manner throughout the test. The mean combined efficiency of the pump and engine was 81.77 per cent. This was the only rotary pump tested in 1905 but several tests were made of centrifugal pumps of various types; some of these had extremely low efficiencies. In all the tests every precaution was taken to guard against error; the general method of testing all the plants was the same.

3 The test of the pumping plant of the Neches Canal Company was made more than a year after the Abbeville test. The instruments used were the same in both with the exception that four indicators were required in the latter test; all were carefully calibrated. The current meter had been re-rated.

4 Considerab'e experience with the Pitot tube has given me confidence in its accuracy. I can see no reason why the constant of the tube should change, whether the tube is used in an open channel or under pressure in a pipe, and I would be glad to see experimental information bearing on this point. Professor Carpenter thinks the Pitot tube the probable source of error. If so, what is the probable amount of the error, and why do the current meter readings and those obtained by means of the Pitot tube agree exactly, in the average, with the displacement of the pumps? The two instruments were used in order that results could be compared and checked. Evidently the errors in the current meter and the Pitot tube are the same; in numerous other tests they have given practically identical results. It seems to me probable that both were correct within a possible error of 1½ per cent. Had a weir been used to measure the quantity of water (153 cubic feet per second) an error of 1½ per cent would not have been unusual, in fact that amount of error would be expected. This is all that was claimed for the accuracy of the water measurements.

5 The Connersville Blower Company constructs its pumps with extremely small clearances; the slip must be correspondingly small. The average slip, as shown by readings obtained by the two instruments used to determine velocity in the Neches test, was *nil*. For an intelligent discussion of the slip of a rotary pump, the clearance must be known; even then the slip cannot be computed with accuracy, but this knowledge will aid in forming an estimate, more or less just, of the amount of the leakage. In the pumps tested the clearance between the impellers and the casings or "cylinders" is about 0.024 of an inch. The clearance at each end of the impellers is about 0.012 of an inch while that between the two impellers at their line of contact is about 0.048 of an inch. This information was furnished by the man who assembled the pumps and erected the plant. The dimensions were obtained by actual measurement; the above values are averages. From the known areas due to the clearances and the measured head, the amount of leakage or slip is found by an approximation, necessarily crude, to be about 1½ per cent of the water discharged.

6 The writer must take issue with Professor Carpenter as to the probable mechanical efficiency of the engines at the Neches plant. There are a great many published reports of tests that show mechanical efficiencies of from 93 to 94 per cent for engines with apparently no less friction losses than those of the Neches plant. Reference will only be made to results obtained by Professor Carpenter or in experiments made under his direction. In Volume 16, p. 929, Trans. A. S. M. E., in the report of tests of the Sibley College cross-compound Corliss engine, at full load, the mechanical efficiency is given as 93.7 per cent. The engine has cylinders 9 inches and 16 inches in diameter, stroke 36 inches. The same paper reports tests of a simple Corliss engine in which the mechanical efficiency is as high as 97 per cent. Certainly the tandem compound engines at the Neches plant, running at from 55 to 58 revolutions per minute might have as high a mechanical efficiency as a smaller, cross compound engine running at about 85 revolutions per minute.

7 In the "Sibley Journal," Volume 15, p. 13, a test of two simple engines in the lighting and power station at Cornell University, is reported. The mechanical efficiency of an 85 horse power McEwen and a 25 horse power Ideal engine is given as 94.3 per cent.

8 In the "Sibley Journal," Volume 19 p. 60, is the report of a test made at Cornell on a Reeves simple engine, which developed at a maximum, nearly 170 horse power. The mechanical efficiency when the engine was run condensing, is given as 95.8 per cent in one case and above 95 per cent in four different cases.

9 The engines of the Neches plant had been running for several weeks previous to the test; they were limbered up and in good adjustment; the cards show the load to be well distributed on the two ends. It, therefore, seems to me altogether probable that the mechanical efficiency of the two engines tested was not less than 93 per cent. The combined efficiency of the engines and the pumps was 83.3 per cent. If we assume the efficiency of the engines as 93 per cent, the efficiency of the pumps would have to be about 89.6 per cent. It must be kept in mind that the losses in the suction and discharge pipes, due to friction, to loss at intake and the velocity head thrown away at the end of the discharge pipe, are charged to the pump. The mean velocity is only a little over six feet per second and the losses consequently small; a computation shows the sum of these losses to be about 4 per cent of the total head.

10 Professor Carpenter estimates the friction loss in the gears at 2 per cent; this leaves 4.4 per cent for hydraulic losses in the pumps only. The rotary pump is more nearly comparable to a piston pump than to a centrifugal pump. There may be a little more eddying of the water in the rotary pump than in piston pumps, but in the latter there are often sharp turns to be made in the water passages leading to and from the water cylinders and the valves have to be passed with a small accompanying loss. Pumping engines of the free piston type often have mechanical efficiencies above 95 per cent; the loss being in part on the steam end and in part on the water end. In the case of the rotary pump, the only loss under consideration is within the pump and is exclusive of the friction of the gears. It seems to me altogether probable that the losses in the pumps tested were not in excess of 4.4 per cent.

11 In making comparisons between various types of pumping plants and especially in deciding on the pump to be used in a particular case, consideration must be given to reliability, first cost, and running expenses, the last include depreciation and repair, interest on investment, wages, and cost of fuel. A part of the running expenses will be constant from year to year as they are pro-rated on the investment. The quantity most liable to fluctuations is the fuel bill. In designing a pumping plant all these variables must be carefully considered, each separate problem worked out on its merits, and the plant designed for the highest financial efficiency. The general method of treating the question may be found in Bulletin 183, Office of Experiment Stations, U. S. Dept. of Agriculture. In that publication I have discussed the various types of pumping plants for a particular set of conditions, including oil at 50 cents per barrel. Without going

into further details, the conclusion was reached that a high grade centrifugal pumping plant was the best investment for the assumed conditions. However, during the past year the price of fuel oil has more than doubled, and it was only necessary that oil cost a few cents more than $1 to make the plant using rotary pumps an equally good investment, as the cost of irrigating an acre with the two different types of pumping plants would be the same. As fuel increases in cost the amount that can be saved by an economical plant will become more and more important and the additional first cost justified by the saving in the fuel will be proportionally increased.

12 The experience of the Neches plant would seem to indicate that rotary pumps may be operated without danger of breakage. The pumps have been operated for four seasons, and I am told have often been run above their rated speed of 60 revolutions per minute without developing troubles of any kind. Difficulties due to fluctuations in the suction and discharge pipes are overcome by the air chambers. A small Westinghouse air pump, such as is used in locomotives, supplies air to the air chambers on the discharge side of the pumps. Sight glasses show the position of the water level. No indicator cards were taken from the pumps or the discharge pipes during the test; there was an entire absence of water hammer. The pumps were operated without noise and they were satisfactory in every way.

13 Undoubtedly centrifugal pumps can be forced above their rated capacity much more easily than can rotary pumps, but the increase in capacity is usually attained at a sacrifice of efficiency and cost of operation. However, cases occasionally arise in which this sacrifice is very gladly made.

14 With the flexible couplings described in the paper, there is no danger from breakage in case wood or other hard substances get into the pump. The babbit keys shear and then "howl" as the engine continues to revolve. This has happened several times at the Neches plant and no serious breakage has resulted.

15 The question as to which type of pump is less likely to give trouble is one on which the men who operate the pumping plants do do not agree. Rotary pumps are not "fool proof" but any high grade plant requires skilful handling and it is false economy on the part of the owners to have any but high grade men to run such irrigation plants.

16 Pumping plants using rotary pumps in Louisiana and Texas have made splendid records for fuel economy; the saving in fuel is not entirely due to the type of engine usually employed but is due also to the high mechanical efficiency of the pumps.

STEAM PLANT OF THE WHITE MOTOR CAR

By Prof. R. C. Carpenter, Published in November Proceedings

Prof. R. C. Carpenter I have read with a good deal of interest the discussion by Mr. Warren S. Johnson. I am not able, however, to agree with him in all particulars nor to look upon matters in every way from his standpoint.

2 The title of the paper "Steam Plant of the White Motor Car" is, it seems to me, fully indicative of the field covered. If the paper had been entitled "The White Motor Car" it would have been open to the criticism which Mr. Johnson makes. It is true that the paper relates to the construction and test of the steam plant of the White Motor Car, and furthermore that the test of the steam plant was made when the supporting framework was stationary. The conclusion cannot be drawn from this fact that the economy of the steam plant for the same conditions of speed and load would be different were the car in motion, and I do not understand that Mr. Johnson makes any such claim, for it is evidently true that the economy would be the same were the steam plant mounted or stationary, provided the other conditions effecting efficiency, such as load and speed, were the same. I think, however, that the object which Mr. Johnson had in mind was to get on record such results as were accessible regarding the operation of the car.

3 It is quite evident that a road test of the car cannot give the fuel economy or efficiency of the steam plant based on definite units or units which are comparable in any way with those used for the measurement of power. A road test could give the fuel consumed per mile on level roads and in hill climbing, but this in turn is very much affected by the character and kind of the road, by personal management, by the condition of the tires, and by very many things which the makers of the vehicle cannot control. The results are also indefinite because of the varying quality of the surface of roads and the varying amount of power required for traction, and at best it can only give the results obtained in the paper reduced by the friction of the running gear. In another form Mr. Johnson may be said to criticise the paper for using a method of obtaining definite results instead of for using a method of obtaining indefinite results and results which, from a scientific standpoint, mean absolutely nothing. I know, however, that Mr. Johnson did not intend to have his statement interpreted in that manner, and that he desired to call the attention to the importance of road tests for a road motor car.

ROAD OR RUNNING TEST

4 Despite the indefiniteness of the results, a great deal of useful information in a comparative way is to be obtained from a road test, especially if a number of cars are made to pass over the same road and the conditions of loading and fuel consumption are carefully observed. At the time of the test of the steam power plant, which is described in the paper, was made, this engine had not then been applied, except for a few preliminary runs, to a car, and consequently the data which he desires could not at that time be given.

5 Respecting this particular engine, in the 1907 car, I have the results of only one road test, and that test was made by Mr. Walter White in England. The general results of that test show the following: Distance traveled, 212 miles; time, 3 hours and 15 minutes; gallons of gasolene, 23.5; weight of machine complete including fuel, 3490 pounds, number of passengers, 5; estimated weight of passengers, 800 pounds. Total weight carried, 4290 pounds.

6 There are numerous records of the performance of the 1906 car with an 18 horse power engine and of the 1905 car with a 16 horse-power engine. In the two gallon efficiency contest, which took place in New York early in 1906, a large number of gas engine cars and two White steam cars entered into the competition. The results of the trial of the 1906 White steam car were as follows:

No. of Car	Weight loaded	No. of passengers	Miles on 2 gallons	Miles per gallon
53	3225	4	23.80	11.90
56	3370	5	22.51	11.26

7 I have made two road tests with a 1905 car. The first was conducted over the ordinary roads of eastern and central New York, during which the Catskill range of mountains was ascended and crossed. The car contained four passengers and luggage and had a total weight of 3020 pounds. The total mileage was 337 miles; the total amount of gasolene consumed was 33 gallons. This would indicate a mileage of 10.2 miles per gallon of gasolene. The average speed maintained for the entire distance was very nearly 18 miles per hour.

8 In a second test made with the same car with two passengers and with a total weight of 2680 pounds, I traveled a distance of 108 miles with a consumption of 8.1 gallons of gasolene. This corresponds to a distance of 13.3 miles per gallon. The latter test was made over an excellent road with very little gradient and with a rather

better burner than that used in the first test which I made. The average speed during this last run was very nearly 21 miles per hour.

9 The above figures from road tests, while not very extensive, will probably be sufficient to give a good idea of the actual performance of the White motor car in conveying loads over different kinds of roads.

10 Mr. Johnson's statement that the test runs were not made under automatic control is not true; the automatic means were employed to quite as great an extent as in the motor car when on the road. The fuel supply was regulated during the tests entirely by the automatic thermostatic control as described. The water supply was controlled partly by hand during the test, as would be the case on a road test with the control means of the 1906 car. The statements in the paper respecting these conditions in the test show that the automatic devices were used in practically the same way as in the operation of the car, although the hand control was used more for the reason that during the test it was necessary to maintain a uniform loading condition which is not material and which does not occur in driving a car over roads.

THE BOILER TEST

11 Mr. Johnson's statement that a flue temperature of 543 degrees F. is abnormally high for the best efficiency of steam boilers does not agree with my experience, nor does it agree, so far as I know, with the statements made in any reliable text-book on this subject. I do not believe that the addition of an extra tube to the boiler, which would have increased both its height and weight, would have added materially to its efficiency. The test figures on another boiler which Mr. Johnson gives, and which I understand had this additional heating surface, do not indicate any great gain over what was obtained with the cheaper and lighter boiler. It is not questioned that the addition of more heating surface would have given a slightly higher efficiency, but I do greatly question whether such efficiency would have been commercial and whether it would have been worth the expense required to obtain it. My own opinion is that a single tube boiler, which reduces the temperature of the flue gases to 300 degrees F., is unduly large for motor car construction. In fact I do not know of a stationary plant which would consider reduction of flue gases to 300 degrees F. as good practice.

12 Mr. Johnson refers to a method of calculating the thermal loss from the velocity of the flue gases. He does not state how such a calculation could be made if the velocity were known. Such a scheme

would be extremely awkward if of any practical value whatever, and entirely unnecessary since the test which was made of the boiler obtained those thermal losses in a much better and more accurate way.

13 Respecting the advantages of the construction of the boiler itself over the one referred to by Mr. Johnson, I have nothing to say since I am not familiar with the construction to which he refers. It was not the object of this paper to compare one structure with another but merely to show the actual results in definite measures which certain structures produce.

14 Mr. Johnson states in effect that the generator of the White steam car does not represent a new system of producing and generating steam. He does not, however, state any references to earlier makes of boiler construction whereby the water could be fed into the top of the boiler and the steam drawn off at the bottom in which the water was maintained at the upper level of the boiler by a system of bends or traps. I have spent some little time since his statement in looking up the history of this important invention, which has revolutionized the production and use of steam in the motor car industry, and I am not able to find that previous to White's invention in 1898 this construction was ever made or even suggested by anyone else.

15 It is very easy and very common to decry the importance of great inventions and also to give credit for their production to nameless inventors, and such statements frequently detract much from the reputation of an excellent invention. In this particular case it is very certain that White was the first to put into practical commercial use a boiler with the peculiar construction of his steam generator, and so far as I can learn he was also the originator of such a system for building and operating steam boilers. Boilers with a downward circulation of water built on different lines and working on different principles, some of which doubtless have considerable merit, have been described, but none of these boilers could produce the results which White obtained from his construction.

METHOD OF MAKING CAR TEST

16 Mr. Johnson suggests the making of a car test, on a dynamometer in which I take it the rear wheels are intended to be supported by traction wheels which are connected to a Prony brake. Such a device is practicable and has recently been installed in Sibley College; this will give the friction of the various parts of the car and also its performance under different conditions as stated. It will not, however, give the efficiency of the engine and boiler, which was the object

of the test. It would give a mixed result combined in an indefinite way of the power plant and running gear and is of little use as a reference. There are so few standards of comparison that the results called for by Mr. Johnson would at the present time be of little value except to motor car builders, but during the coming year our program includes the testing of a number of cars in such a manner.

ADVANTAGE OF THE COMPOUND ENGINE

17 The excellent results which were obtained in the test were doubtless due in great measure to the high degree of superheat and to the high pressure of steam. I do not believe, however, that results could have been obtained which approximated these if a simple instead of a compound engine had been used. Mr. Johnson points out that a certain railroad has not found the compound locomotive as satisfactory as the simple engine; an investigation would also show him that certain other roads have found it more satisfactory. This merely tends to prove that we cannot decide whether a simple or compound engine is preferable from locomotive practice; as a matter of fact, the locomotive works under very different condition since its steam pressure rarely exceeds 200 pounds. It may be urged that as superheated steam prevents cylinder condensation, it does away with the gain due to compounding, but on the other hand it will be found that with as high steam pressures as 600 pounds per square inch it is nearly impossible to get sufficient expansion in the steam cylinder of a simple engine to give high economy. For that reason the compound will in my opinion give much more efficient results. It would be interesting, however, to test out a simple engine under the same conditions of steam pressure and superheat and compare it with the work done by the compound.

18 In marked contrast to Mr. Johnson's views I present a quotation from a letter by Mr. E. C. Walker, another builder of steam cars, who is designing a triple expansion engine:

> We have read with great interest in the December 19 issue of the "Horse'ess Age" the results of tests made by you of the steam power plant of the White automobile as set forth in a paper read at the December meeting of The American Society of Mechanical Fngineers. We wish to congratulate you on the inestimable value of this work which you have conducted, and without hesitation will say that we consider this the most valuable paper on steam engineering, as applied to the motor car, that has ever been put in print.
>
> Do you think that the use of a triple expansion engine having cylinders of correct proportion, if used in combination with single tube boiler, or so-called flash boiler, would result in any marked degree of economy? Do you consider that the economy would justify the additional cost of manufacturing such an engine, and

do you think that such economy would more than make up for the increase in weight of the engine, due to the addition of a third cylinder? We have been working in designing a triple expansion engine, intended for automobile service and to be used in combination with single tube boilers which we manufacture.

19 My reply to Mr. Walker encouraged him to expect greater economy, but it was not of a nature to give him any positive opinion as to the practical value of such an engine on a motor car.

20 The experience with the White compound engine has indicated that it had no practical disadvantages as compared with the simple engine, but that on the other hand it had many advantages, as it had proved not only more economical but it was more easily kept in repair and practically was superior from every standpoint.

21 In connection with this paper, attention should be called to a paper by Mr. Thomas Clarkson on "Steam as a Motive Power for Public Service Vehicles," which was read at the November meeting of the Institution of Mechanical Engineers, England. Mr. Clarkson is a builder of steam cars for public service work; the paper referred to gives the details of his system of construction, and states the advantages which have resulted from its use, as compared with an internal combustion motor, and gives the costs of operation of certain cars in a public service for carrying passengers.

22 In the discussion of this paper by Col. Crompton, attention is called to the fact that Mr. White in his steam car has succeeded in getting nearer the internal combustion engine in regard to actual efficiency of fuel than anybody else with whose record he was acquainted.

23 Col. Crompton also states that he knew of White cars weighing $1\frac{3}{4}$ tons fully loaded that had been able to run at full speed with light loads 14 to 15 miles per (English) gallon of benzine. Respecting this performance he states:

> It was a new era in the use of steam. And the friends of that old and tried servant had now to take up the problem and work it out on the lines that had been followed so successfully.

24 Mr. Clarkson's vehicle is adapted to operate with kerosene oil and a considerable portion of the discussion relates to the advantages and disadvantages of kerosene or, as the English call it, paraffin, as compared with gasolene or petrol.

25 The discussion by the chief inspector of the new Scotland Yard, Mr. Bossom, indicated that while paraffin had a much higher flash point, yet in practice it was actually more dangerous than petrol or gasolene. The statement was made and not denied that with paraffin

the vehicle moved two miles per gallon while with "petrol" it moved seven miles per gallon.

26 The danger in the use of kerosene came from the light vapors driven off by the burner taking fire outside of the boiler structure. It was stated that while gasolene was much more volatile, in practice more precautions were taken in handling it and fewer accidents resulted. Another objection to the kerosene was the long time required to heat up, the great amount of smoke, and the bad smell.

27 The above statement is interesting as being quite contrary to the opinion held generally respecting the use of kerosene as a fuel. The particular danger it was claimed came from the fact that the kerosene was greatly heated before it entered the burner, which rendered it anything but a safe fuel.

CONTROL OF THE 1907 CAR

28 Since the paper was written, a new system of automatic control for the supply of steam and water has been adopted and put in use on the White steam car. In the new system of control a constant steam pressure is maintained on the boiler regardless of the conditions of the load. The hand by-pass for the feed water has been removed and practically the only thing required of the operator is to steer. In the new system the feed of fuel is controlled by the action of a thermostat on a motor operated by the pump and termed a flow regulator. The water is controlled by a pressure regulator as in the old system.

29 Whenever the steam pressure exceeds the normal working pressure, say 600 pounds per square inch, the water regulator opens a by-pass valve and diverts from the generator the stream of water from the pumps. When the steam pressure falls below 600 pounds, the regulator again acts, closing the by-pass valve, and the water from the pumps is again directed to the generator. The regulating system up to this point is identical with that used on White cars for the last six years.

30 From the pumps there are two separate and distinct paths by which the water may reach the generator, either through or around the "flow-regulator."

31 The flow-regulator, A_4, is a compact and simple motor operated by the pump which may be said to have two distinct functions. Its first function is to open and close the supply of gasolene to the burner, which it does in this fashion; when water is passing through the flow-regulator, the water forced in by the pump moves a piston, against the action of a spring, and the motion of this piston is utilized to open a valve B_2 in the gasolene line. When water stops passing through

the flow-regulator (which will happen whenever the engine stops or whenever the pressure exceeds 600 pounds), the piston is no longer under pressure and the action of the spring returns it to its original position, this motion serving to close the valve in the gasolene line, thus shutting off the supply of fuel to the burner. Second, it is so constructed that it will permit only a definite quantity of water to pass through it, any excess being returned through a by-pass valve to the water tank.

32 The accompanying diagram of the new control system, which is merely a modification of the old one described in the paper, may make its mode of operation clear. In the diagram, A_1 is the pump, A_6 the generator, A the water tank, A_4 the flow regulator or motor, B_2 the fuel valve, A_5 the thermostat, A_2 the water regulator.

33 The water regulator A_2 is operated by pressure to turn on or off a by-pass pipe leading back to the water tank. If the steam pressure exceeds 600 pounds the by-pass valve is opened and the water returns to the water tank; if in excess of that it is closed, the entire water delivered by the pumps is forced toward the generator. The feed water line has two branches, one flowing through the thermostat A_5, the other through the motor or flow regulator A_4, which branches unite again before reaching the generator. The branch leading through the thermostat has its flow controlled by a valve which is opened or closed by the thermostat and which acts by the temperature of the steam. If the temperature of the steam is too low, the thermostat serves to close the passage to the branch around the flow regulator and divert it into the flow regulator; this causes more pressure to act to move the piston in the flow regulator a greater distance which in turn opens up the fuel valve B_2, and lets on more fuel. If the piston in the flow regulator moves too great a distance, it opens a by-pass valve in pipe A_7, and allows the excess water to return to the water tank and prevents flooding of the boiler. If the temperature rises, the reverse operation takes place and the fuel is closed off. The new regulator differs from the old one in this way, that whereas in the old system, the thermostat controlled the fuel valve directly, in the new system it controls it indirectly through a motor, thus giving it a greater range of motion in a more positive manner.

GENERAL ADVANTAGES OF STEAM CARS

34 I have been asked several questions respecting the advantages which steam possesses as motive power for cars. This consideration has not formed any part of my paper and it is evident to any person who considers the subject that each and every kind of power will

possess peculiar advantages for certain conditions or for certain demands.

36 In an article published in the "Review of Reviews," January, 1907, by Harry D. Haines, the following statement, which will, I think, be generally concurred in, is made respecting the steam machine.

> The steam machine has these advantages: little jar or noise, ease of control, and simplicity in operation, inasmuch as a single throttle controls all speeds, and thus a more elastic power is secured. Gears are eliminated and so is the possibility of putting the car out of commission by stripping them. The steam car, with its direct drive, makes possible greater simplicity of construction and a reduction of repair bills in the absence of ignition systems and their troubles, as well as the further absence of "overheating" and the troubles due to carbonization of lubricating oils in gas engines. Steam is a known power and is more generally understood and more easily repaired when out of order. The bugaboo of burned out boilers and other boiler troubles has been eliminated by the use of the flash boiler, and all adjustments and regulations are now automatic, depending only on varying temperature and pressure. In a steam car wear and tear on the machinery is saved by the fact that the engine never races and does not run when the car is standing still. The engine has greater elasticity, inasmuch as it is possible to increase steam pressure and the consequent power enormously when bad hills or roads are met with, giving a valuable reserve power. The absence of smoky exhausts, of "back firings" of dirty motors, and the noises of worn ones are claimed as advantages for the steam car, as is the fact that the cost of fuel consumption is proportionate to the power developed. As a final clincher the cheapness in first cost is added.
>
> On the other hand, the steam machine requires time to be got ready for road work in waiting to get up pressure. The need of extinguishing the fire when the car is stopped for any length of time and relighting it again is quoted against the steam car, but this to a great extent has been overcome by improved burners and pilot lights. The limited water and fuel capacity, increased gasolene consumption, and the trouble from clogging of valves or failure of pumps are also considered disadvantages, and it is further maintained that the results of neglect are more serious in a steam car, and that the necessity of using soft water at times causes annoyance, as does the freezing of pipes in cold weather. Last but not least, many people are influenced by the greater danger of the destruction of the car by fire in case of accident. This is in reality the greatest argument against the steam machine, for if the car is involved in a smash-up, or is ditched, the gasolene feed-pipe is apt to be broken and then the gasolene ignites from the burner.

37 If an amount of steam greater than that which corresponds to the water coming to the generator is drawn off, the steam pressure will, under these circumstances, tend to fall off and the temperature of the steam in the generator will tend to rise above the normal, because the fire will remain "on" since, as has already been stated, the capacity of the burner is in excess of that necessary to evaporate the water going into the generator by way of the flow regulator; as soon as the

temperature of the steam exceeds the normal, the thermostat acts, opening a valve which permits water to be diverted from the flow regulator or motor, thus closing the fuel valve. Under these conditions the temperature and pressure immediately become normal and the thermostat again acts, shutting off the auxiliary water supply.

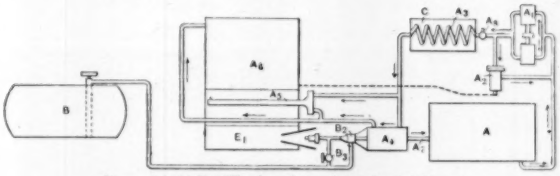


Diagram of the Modified White Steam System, showing the principal water and fuel connections, together with the automatic control devices.

A. Water Tank.
A1. Pumps.
A2. Water-regulator.
A3. Coil in Feed-water Heater.
A4. Flow Regulator.
A5. Thermostat.
A6. Generator.
B. Fuel Tank.
B2. Fuel Valve.
B3. Hand Valve for starting.
E1. Burner.

FIG. 1 DIAGRAM SHOWING THE NEW CONTROL SYSTEM FOR THE SUPPLY OF STEAM AND WATER

38 Respecting the disadvantages which are stated, the one in regard to the use of soft water I have found does not occur with the White steam car. No difficulty whatever has been experienced due to boiler scale or even to oil in the water, which fact I have already referred to in the body of the paper. I have also found no disadvantages resulting from limited water or fuel capacity, nor has any trouble been experienced due to the clogging of valves or failure of pumps. The White car which I have been operating has water and fuel capacity sufficient for 150 miles, which is practically the same as that of most of the gasolene cars. There has also been no trouble experienced from increased gasolene consumption which on the whole does not seem to average in practice much greater than that of gasolene cars with the same carrying capacity, and is more than offset by the smaller bills which require to be paid for tires.

39 I agree with Mr. Haines that the greatest argument against the steam car is the danger from the open gasolene flame in case of accident. This danger in the 1907 White car has been greatly reduced by the heavy construction of pipe and gasolene tank, by entirely enclosing the pilot lamp, and building it generally in such a manner that the gasolene pipe or tank could not be ruptured by an accident, and by special features which confine the flame under all conditions to the burner. Considering the number of these cars in use, the accident from the open flame has been very rare and except in a few cases has generally been due to gross carelessness on the part of the operator or those handling gasolene.

BOILER AND SETTING

By A. Bement, Published in November Proceedings

Mr. A. Bement There has never been an agreement between Professor Kent and myself as to the capacity of the tile roof furnace for smokeless combustion. I am disposed to consider that it is much greater than he, and to support my opinion, I will say that in no case whatever within my knowledge, when coal from Iowa, Missouri, Illinois, Indiana, and Western Kentucky, has been burned in an apparatus of this kind, has there been any smoke, irrespective of thickness of fire or strength of draft, although minimum draft has not been below 0.075 of an inch of water over the fire in any of the cases observed. This, of course, is very low, and should it become lower, whether there would be a tendency for carbon to go free or pass away in the form of CO_2 is an unsettled question. At all events, with ordinary chimney heights and draft conditions, thickness of fire or any amount of air supply has cut no figure in the results so far as smokelessness is concerned, and means to study this has been afforded by the Harrison Street plant of the Chicago Edison Company, which has been equipped with these furnaces for several years.

2 As to the provision for increased air supply, I really wish that it was necessary. It is not, however, because there is always an abundance of air; in fact, an undesirable excess with every chain grate fire, and if the stoker could be so arranged that there would naturally not be a sufficiently large air supply, the most important step in the solution of the problem of high efficiency would be assured, because it would then be a very simple matter to provide means whereby additional air could be admitted according to requirements.

3 Relative to the per cent of CO_2 obtainable, according to analysis, it has been as high as 16 or 17 per cent in this type of furnace when equipped with a chain grate stoker, with a draft, however, not to exceed 0.15 of an inch of water over the fire, and an econometer attached has shown it above 17 per cent for a period of four hours, and, at occasional intervals under peculiarly favorable conditions, the econometer needle has swung as high as 19, and calibration of the instrument between 4 and 14 per cent CO_2 showed it to be quite accurate. Above this point, it was not feasible to check it by analysis, although there was no occasion for its accuracy to be doubted above 14 per cent, but when drafts became stronger, the CO_2 dropped and excess of air increased quite rapidly.

4 In reference to the balance draft insuring complete combustion with a smaller air supply, the meaning is that in the case of the tile

roof furnace described, there would be no question concerning complete combustion under any operating conditions, and by the use of a combination of forced and induced draft which would bring about the balanced condition, there would be less leakage of air into the furnace chamber. The statement would have been more to the point had it been framed as follows: that the use of the balance draft prevented some leakage which would occur under conditions of a partial vacuum in the chamber. The two examples of results secured in the tests with and without the balance draft, are, of course, very low, and were only quoted because they were absolutely comparable, boilers and operating conditions being exactly the same in each case with the exception of difference in draft, and the data were selected from other records which had been produced for other purposes than that of this paper.

5 Mr. Carey calls attention to the fact that no mixing devices are placed in the furnace chamber beyond the bridge wall for the purpose of effecting a mechanical mixing of the gases. This is because experience has demonstrated that it is not necessary when a chain grate stoker is used, on account of the size and massiveness of the flames not being sufficiently great to prevent their burning away naturally before the exit from the furnace is reached. With different conditions of coal feeding than that assured by a chain grate, it is most essential that these mixing devices be employed as has been fully discussed elsewhere.[1]

6 There are two ways of reducing leakage into boiler settings; one, to lessen the vacuum therein; the other, as suggested by Mr. Carey, to use better settings, both of which schemes can be employed at the same time with advantage.

7 I am pleased to have the testimony that the application of fire brick tiles over the fire is not new, because it helps to demonstrate the fact that there is no uncertainty as to their usefulness and that their value has been proved. The form of furnace roof which the Babcock & Wilcox Boiler Company has used, however, is more on the order of the T tile baffle used by the Heine Boiler Company, and is applied to its marine type of boiler for the purpose of obtaining some approximation to the heat distribution over the tubes afforded in its regular standard form of stationary boiler, rather than for the purpose of securing any benefit from better combustion.

8 It is my understanding that a considerable number of people have designed a tile, either for the same purpose or similar to that shown by me in Fig. 2, and I am glad that it is so, because, as before

[1]Journal Western Society of Engineers, December, 1906.

mentioned, it demonstrates the fact that the particular improvement which I am advocating is not made up of untried and uncertain features.

9 As to the matter of using 3.5 inch tubes in the bottom row, while those in the rest of the boiler are all 4 inch, I recognize that this is a disadvantage, justified only in view of the fact that it is accompanied by the advantage of offsetting the harm occasioned by the two sizes. The only substantial objection is that two turbine cleaners are required; also, that if tubes are carried in stock, one or two 3.5 inch should also be on hand, and that a different sized expander will be required in replacement. The cost of these three features is readily calculated and determined, and when once provided for, cannot strictly be considered as a continuing source of expense. On the other hand, the advantage to be derived from the use of 3.5 inch tubes is that a thicker necked tile can be used, insuring a greater life to the furnace roof and decreased cost in its application, because with the Babcock & Wilcox type of boiler, the horizontal tube centers are not exact. They may vary as much as half an inch, therefore tiles must be made so that they will compensate for this variation, and with 4 inch tubes the space between them is, if anything, rather narrow for the most desirable results, although a considerable number of the Babcock & Wilcox type of boilers have been changed and some new ones built, wherein the tile roof furnace has been very successfully applied to 4 inch tubes.

10 I am much pleased to have Mr. Bryan's testimony to the effect that so many features of the design submitted are old and devised by other people, and I thank him for presenting such valuable information, although in this connection I should properly call attention to the fact that it was not the intention to intimate that the design which I submitted in Fig. 1 was new, but rather that it was an improved form, and as such, only in contrast with prevailing practice could it be considered as a novelty.

11 As to the accessibility of baffles and effect of high temperature there has never been any difficulty encountered. The rear baffle would be most subjected to heat, and for that reason I suggest the extension of the furnace roof a considerable distance beyond this rear baffle, so that the hot gases will be compelled to travel a considerable distance across the tubes before they can reach this baffle, or in other words, instead of setting the rear baffle at the end of the roof, I place it a considerable distance forward. As to the matter of accessibility, it has seemed to me desirable to add a door in the setting, giving access to the chamber between the bottom row of

tubes and those above in the rear part, as shown by the entrance door in Fig. 8.

12 Relative to such an arrangement as I have shown having the effect of cutting down the capacity in regular work and particularly at overloads, Mr. Bryan is greatly mistaken, because the opposite is the fact, the effect being to increase the capacity rather than to diminish it. This is because the high furnace temperature causes more coal to be burned with the same strength of draft than would be the case if the fire were directly exposed to the boiler.

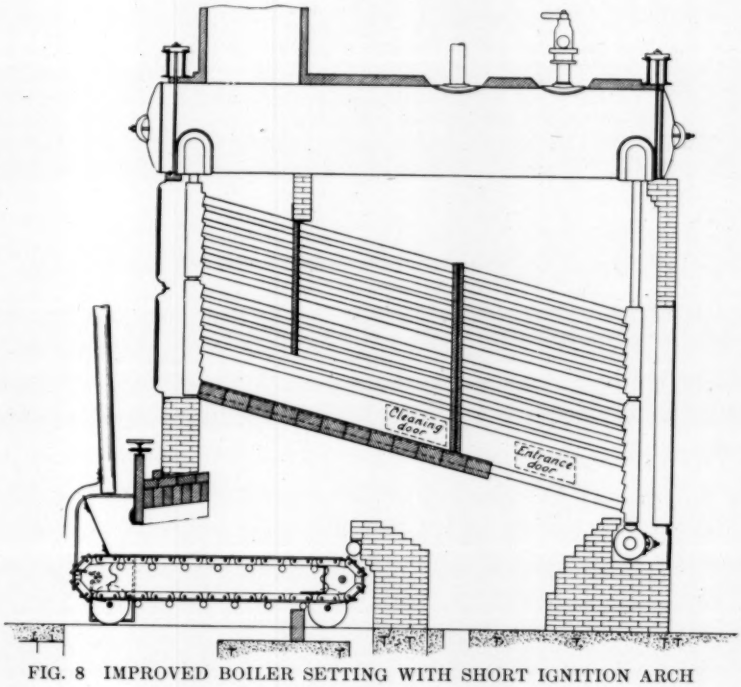


FIG. 8 IMPROVED BOILER SETTING WITH SHORT IGNITION ARCH

13 The criticism applying to the chain grate is that it is not suitable for wide fluctuations in load, and as far as this matter is concerned, no furnace is, strictly speaking, but I dare say that it would be conceded that a hand fire approaches it as closely as anything, therefore, I would say that in a large rolling mill plant in Chicago chain grate stokers were installed under water tube boilers to produce steam for mill engines, which arrangement everyone will, no doubt, concede as affording an intermittent and irregular service. In the same plant are similar mills operated by hand fired boilers, and the records show that the chain grate apparatus is more economical in

fuel than are the hand fired, but it is not necessary to go into details on this score, because the chain grate stoker is handling every variety of steam load, and doing so successfully. Nearly all of the electric and street railway current in Chicago, as well as other important cities, is being generated on chain grate stokers with great success.

14 Mr. Bryan states that boiler builders of wide experience have spent thousands of dollars in experimental research, and have adopted arrangements of baffling which best meet average conditions of service. The intimation apparently is that they have arrived at better conclusions than I, and on this point I presume my opinion may be considered as having been given and no final answer is obtainable at the present day, therefore, I propose to Mr. Bryan that we postpone the settlement of this feature until some time in the future, and then it will no doubt be apparent who is right.

15 In reply to Mr. Whitman my remarks about patented features of the apparatus described, apply only to the illustration, Fig. 1, and I did not consider the balance draft as any portion of it, therefore, I am very glad to make this point clear, because it is not the intention to mitigate in any way the value of the various patents mentioned.

16 In reply to Professor Jacobus' remarks, defending the Babcock & Wilcox Company's setting, I will say that experience, covering six or more years, has demonstrated that his opinion regarding the cost of maintenance of brick work has no foundation in fact, and the reduction in draft, which he believes to exist, does not occur. I am sure that he has not seen any of the plants in the West which he says produce no smoke. If he had he would scarcely have made the assertion.

17 The long ignition arch adds to the length of the setting, and consequently in many cases necessitates a considerable real estate and building investment cost. For this reason I prefer the design shown by Fig. 8, which illustrates a boiler recently made to my specifications.

18 In reply to Professor Hitchcock, I will say his conclusion that the presence of a tile furnace roof causes a reduction in capacity is probably based on insufficient data, the cause being elsewhere, as in an extended experience I have failed to observe such effect.

A HIGH DUTY AIR COMPRESSOR

By Prof. O. P. Hood, Published in November Proceedings

Prof. O. P. Hood The main concern of this reply will be to substantiate the results of the test where called in question and to supplement some of the too brief explanations of the paper. Attention will also be called to what seems a wrong interpretation or a misuse of the results in order to make more clear the unique performance of this engine.

2 Mr. Barrus calls attention to the large amount of moisture in the steam, and in preparing for the test this was a surprise and was carefully investigated. It was recognized that much depended on the results of the calorimeter, and in the year preceding the test several measurements of steam quality were made with different sampling tubes, different calorimeters, and from two places in the steam pipe. During the test the sample was drawn through a perforated tube of the form recommended by Professor Carpenter and placed where for some five feet the steam had been flowing in a vertical pipe giving reasonably parallel stream lines. The position of the sampling tube seemed favorable for a fair sample, a Carpenter throttling calorimeter heavily covered with magnesia covering, was used, and the "Green" thermometer stem well protected. All of the several tests with other calorimeters and tubes led to the same conclusion of very wet steam. That the steam was of variable quality and wet was also evident to observers without instruments, as when moisture was excessive the steam pressures in the reheaters would vary and valve stems and packing show wet, so that from the very considerable amount of evidence gathered on this point I feel that "the reliability of these data has been carefully substantiated," and that there is no need to consider the standard coal increased to 1.16 as Mr. Barrus thinks possible.

3 If the author advanced a reason for the high moisture content of the steam passing a good separator, it would be along the line of calling attention to the fact that the carrying power of the more dense steam is very much greater at 250 pounds than at 150 pounds, and that a separator or a dry pipe which may work well at the lower pressure may have reasons for failing when the carrying power of the fluid at equal velocity is greatly increased. I know of no proof of this proposition but experience with pressures above 150 pounds is limited. Mr. Barrus is in error in stating that "the heat consumption . . . . equivalent to 1.016 pounds of standard coal per indicated horse power is based on the quantity of dry steam passing to the

engine through the throttle valve." It is not based on the dry steam fed to the engine but on the heat content of the wet steam, the engine being charged with the heat contained in the 5.74 per cent of moisture carried with the steam. Mr. Barrus also makes a comparison between results which he thinks he could obtain from a triple expansion engine with the actual results shown by this test.

4 The object of the paper was not to argue the relative merits of multiple expansion engines, but to state that a duty had actually been obtained which was considerably higher than had ever before been reached by any steam engine. However, I am of the opinion that Mr. Barrus has made a serious error in assuming, as a basis of calculation, that by raising steam pressures from 150 to 250 pounds and adding a third cylinder to a compound engine "that such cylinder would add one-third to the power developed without increasing the consumption of steam." If this were possible, there would be many more advocates of such high pressures for, as a matter of fact, none of the anticipated difficulties have been experienced in handling as a commercial proposition steam at 250 pounds pressure in this plant, the engine of which is under discussion.

5 Engines which are equally effective in changing heat into work will do work in proportion to the theoretical possibility of the heat cycle they have to use. With steam at 150 pounds pressure this theoretical possibility is the return of 26.3 per cent of the heat used in the form of work and using 8.8 pounds of steam per horse power per hour. Raising the pressure to 250 pounds changes these figures to 28.5 per cent and to 8.03 pounds of steam. The high pressure steam therefore is able to yield 2.2 per cent more of its heat into work, or a gain of about 8.4 per cent, and would do this with 0.77 of a pound less of steam. Using equal weights of steam (which is not equal heat or coal quantities) the gain would be 9.5 per cent.

6 Neither of these figures leads one to expect 33 per cent more work from the high pressure steam nor are there practical considerations which will greatly modify this conclusion. The expansion of steam from 250 pounds to 150 pounds is not sufficient to yield one-third of the work to be obtained in expanding from 150 pounds to the usual terminal pressure of a condensing engine, and I therefore believe that the computations based on such an assumption are very far from conservative and are impossible.

7 The record which is cited for a cross compound engine is itself remarkable, and had the regenerative feed water system been applied to this engine together with the high pressure and added cylinder, a yet more remarkable result would have been obtained.

8 It is not apparent, however, that the low coal consumption of 1.18 pounds which Mr. Barrus reports was obtained in a manner comparable with "standard coal" assumptions. It is quite likely that in so good a plant test, the combined boiler and economizer, together with coal of high heat content yielded to the steam much more than the conventional 10,000 B. t. u. figured for the other engine, and the two figures are therefore not comparable. In the absence of a plant test of the quadruple engine the heat furnished by the boiler per indicated horse power for the two engines are the only comparable items and this Mr. Barrus does not give for the compound engine. For the quadruple engine this is given as 169.29 B. t. u. per minute per indicated horse power or 189.7 B. t. u. per horse power of useful work delivered.

9 That the several heaters, etc., required in the feed heating system have the appearance of complication there can be no doubt, but the complication is more apparent than real. The added heaters and pumps have proved to be not so complicated as the economizer, and familiarity has removed its appearance of complication. In actual running the heaters are automatic and in the two years' use have required less attention than the economizer.

10 The suggestion to heat the feed water to the high temperature attained by using an economizer instead of the several heaters is a most natural one and looks promising until one begins to figure on the actual conditions. If 10,000 B. t. u. per pound of coal are put into the steam and the engine uses 852 B. t. u. from each pound of steam, then the boiler must have handled 11.75 pounds of feed water per pound of coal. The flue temperatures at this plant are about 475 degrees which is much lower than is assumed when great savings are credited to the economizer. Assuming that 20 pounds of gases are made per pound of coal and that gases leaving the economizer have the same temperature (T) as the heated feed water, then the heat given up by the gases would be 20 (475°–T°) 0.24 B.t.u. The gain in heat of the 11.75 pounds of feed water taken from the preheater and raised to the temperature (T°) could not exceed 11.75 (T°–114°). From this the possible temperature of feed water and gases leaving the economizer is found to be about 219 degrees. It is safe to say that such a low temperature of flue gases could not be reached, nor could the feed be raised above 200 degrees, while by the system adopted a temperature as high as 335 degrees was reached and a small further addition made in the economizer.

11 If the feed water had been sent to the economizer from the preheater and raised to a possible 200 degrees, the boiler would have

had to add 988.7 B. t. u., instead of the 852.3 required by the heater arrangement used.

12 It would have been necessary to have added 11,600 B. t. u. per pound of coal instead of 10,000 to be equally efficient, or to have reached a boiler efficiency of 86 per cent. Such a boiler efficiency has not been reached. In a plant showing very low heat consumption per indicated horse power and good boiler efficiency, the field for an economizer is much narrower than with the average plant. There may be several sources of relatively low grade heat about a plant, such as exhaust from independent auxiliaries, as Mr. Lewis suggests, or the gases just discussed, but the range of their availability for feed heating being "low grade" heat is considerably below the high temperatures reached by the heaters under discussion.

13 It seems therefore that with a well designed boiler plant an economizer can not alone equal the results obtained by this feed heating system. In view of these facts, Mr. Barrus makes too broad a statement in saying "that it is certainly a wrong principle to take heat out of the steam in its passage through the various stages of the engine and deprive the engine of that much power." This would be equivalent to saying that it was a wrong principle to take the work which has once been produced by adiabatic expansion of a gas and in the Carnot cycle put it back into the gas as heat in adiabatic compression. There is an exact parallel in the two cases and yet there is no principle more firmly established than that such a procedure leads to the highest thermal results. An engine using steam at 256 pounds pressure and rejecting all its heat to the condenser, usually works with a cycle returning a possible 28.4 per cent of the heat as work, and if it could use the most efficient cycle 34 per cent is the theoretical limit. The system of taking heat out of the steam in its passage through the various stages of the engine to put into the feed water makes a cycle intermediate between the two above cases.

14 To many, such an explanation is lost, but it is easily grasped that an engine using 11.23 pounds of steam per indicated horse power and returning 30 per cent of this direct to the feed water with all of its latent heat, would use a smaller amount of heat than if all the steam had been rejected through the condenser.

15 Mr. Saunders' discussion attempts to focus attention upon the steam consumption per indicated horse power of two engines, and completely ignores the fact that the steam consumption is not a proper measure of the performance of an engine. It ought not to be necessary to point out the fact that an engine uses heat furnished

by the coal, and uses steam merely as a convenient carrier for that heat. The quantity of heat used is the measure of its call upon the coal pile, and this is not proportional to the quantity of steam used except in the case of engines using steam of the same heat content and returning feed water at the same temperature.

16 A proper measure of an engine's performance therefore is the amount of heat required to develop a horse power rather than the weight of steam carrying this heat. If one wished to illustrate this thesis, no better comparison could be made than between the engine of the paper and the engine mentioned by Mr. Saunders, a test of which is reported in the "Engineering Record" of November 8, 1902. This report of a Cooper compound engine with high cylinder ratio shows a steam consumption of 11.22 pounds, almost exactly that of the Nordberg quadruple. The Cooper engine used steam at 172.2 pounds gage pressure at a temperature of 418 degrees and rejected this from the engine at a temperature of 68 degrees. The steam was therefore superheated and contained 1221 B. t. u. per pound, returning only 36 B. t. u. to the boiler.

17 There should be charged to the engine therefore this difference of 1185 B. t. u. per pound of steam and this heat must come ultimately from the coal. The plant had an economizer heating the feed to 182 degrees, and even allowing the economizer to be counted as a part of the engine, the boiler must still supply to each pound of steam 1071 B. t. u.

18 In the Nordberg engine using the same weight of steam per horse power the heat content of the wet steam was 1157.4 B. t. u. of which 305.1 B. t. u. was returned to the boiler, making a net charge of 852.3 B. t. u. per pound of steam used. The Nordberg engine therefore used about 8/10 of the amount of heat, and therefore coal, used by the engine Mr. Saunders compares with it, although each used the same weight of steam per indicated horse power.

19 It is evidently misleading and incorrect to focus attention on the equality of the weight of steam used. A failure to recognize this fundamental thermodynamic principle has led Mr. Saunders to ask for the "weight or volume of air compressed and delivered per *pound of steam* consumed," which is a very misleading basis of comparison to present to a user where the conditions of steam pressure and feed water temperature are unlike. The factor of evaporation for the boiler furnishing steam to the Cooper engine would have been 1.11, while for the other plant it would be 0.883, and the same heat from the coal that would furnish 11.22 pounds of steam as used in the Cooper engine would furnish 14.1 pounds to the engine under discus-

sion. This conclusion agrees fairly well with the coal consumption given for the two engines.

20 The coal used in the test of the Cooper engine was Pocahontas, probably of about 14,500 B. t. u. per pound. The rate of combustion was less than 10 pounds per square foot of grate per hour and flue temperature 448 degrees. The conditions were good for a boiler efficiency above 70 per cent and would therefore put more than 10,000 B. t. u. per pound into the steam or more than "standard coal." If the efficiency of the combined boiler and economizer was 76 per cent, 11,000 B. t. u. per pound would be put into the steam. Using 1.229 pounds of this high grade coal, as reported, per indicated horse power would be equal to 1.35 pounds of "standard coal" for the Cooper engine, to compare with the 1.016 pounds of "standard coal" figured for the quadruple engine or over 30 per cent more "standard coal" for equal work. From the relative quantities of heat used per indicated horse power in the two engines, we would expect 25 per cent more coal to be used by the compound and the 30 per cent above figured shows that the assumptions as to boiler efficiency are not far out of the way.

21 To answer the question as to the amount of air compressed, 6630 cubic feet of free air was compressed from 13.9 pounds to 85.3 pounds pressure by an expenditure of 167,547 B. t. u. contributed from the coal. This is equivalent to 2527 heat units per 100 cubic feet of free air compressed. A plant using heat under the conditions cited by Mr. Saunders where 1071 B. t. u. are used per pound of steam, would have to do this work on 2.35 pounds of steam to equal the above result. The usual bids for doing this work, when called for in this form, run from 3.1 to 4 pounds of steam per 100 cubic feet of air compressed, depending on the steam pressure available.

22 Mr. Saunders also considers it remarkable that the "compression line of each air card at the very beginning of the stroke is quite perceptibly above the atmospheric line." This is true of three out of the four low pressure cards, and the cut is a careful reproduction of the cards as taken. This condition is neither uncommon nor remarkable. An investigation made in 1905 to determine the losses due to faulty valve action in air compressors taken just as they are found in daily running condition, led to the taking of indicator cards from large compressors at eight of the mines of this district. These machines were Nordberg, Ingersol, Rand, and Riedler, and of 34 cards taken from the two ends of 17 different air cylinders, ten of these cards show the same condition as to raised compression line at the beginning of compression. So common an occurrence could not

have escaped for so long Mr. Saunders' attention. The raised toe of the diagram may be due to three causes; retarded indicator movement, due to the elasticity of long string connections, inertia, and elasticity of air columns leading to cylinders, or leakage of discharge valves or piston. Care was taken to eliminate error due to the first cause by using piano wire and steel tape indicator connections.

23 That the inertia and elasticity of air in intake pipes may cause surging of pressure is well authenticated from many cards.

24 Fig. 1 shows a card taken from the inlet chamber *before* the inlet valves, of a 38″ x 48″ air compressor running at 66 revolutions per minute, and Fig. 2 from another of the same size but of different make running at 45 revolutions per minute. The direction of motion of the piston is shown by the arrows. The scale of the indicator

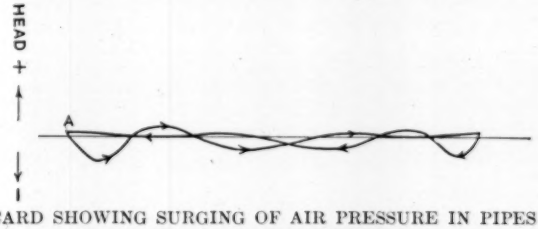


FIG. 1 CARD SHOWING SURGING OF AIR PRESSURE IN PIPES LEADING AIR TO AIR COMPRESSORS

FIG. 2 CARD FROM PIPE LEADING TO PISTON INLET TAPPED NEAR END OF PISTON ROD AT EXTREME POSITION

spring was four. This card shows the wavelike variations in pressure, and that at the termination of the inlet stroke the pressure at each end was above the atmosphere. If the resistance through inlet valves is low this raised pressure is also found in the cylinder.

25 The acceleration of the entering air will follow the same laws as the acceleration of a crosshead and piston modified by the elasticity of the air, but the tendency will be to lower the pressure at the beginning of the inlet stroke and raise it at the end.

26 It is well recognized that pumps with long suction lines may deliver a quantity of water in excess of the geometrical displacement of the piston if no means are provided to overcome the inertia effects of the suction column. The same effect is noticeable in a machine pumping air, although to a far less extent. I am not aware that this effect has been so commonly discussed in print as in the case of pump-

ing machinery, but to show that the same inertia effects are present an illustration, Fig. 3, is given, which was taken in February, 1907, from the high pressure air cylinder of a 25″ x 39″ x 48″ Rand air compressor running at 40.1 revolutions per minute.

27 The point I wish to illustrate is that an air column started into motion may produce higher pressures at one end of that column, when retarded, than at the other end, as this is the point on which Mr. Saunders wishes "indisputable evidence." In the case of the cards which Mr. Saunders criticises, this effect is in the admission column, and I have given two cards from admission columns illustrating the fact.

28 The card from the Rand machine shows the effect more pronounced, being from the discharge column where the density of the material moved is greater. Simultaneous indicator cards were taken from the head end of the air cylinder, giving the full line card

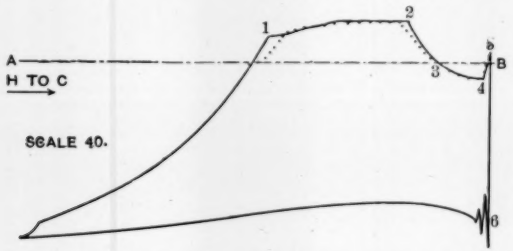


FIG. 3 CARD FROM H. P. AIR CYLINDER FROM DISCHARGE PIPE LINE AND FROM RECEIVER

1, 2, 3, 4, 5, 6, also from the discharge pipe line close to the cylinder, giving the dotted line closely following the line 12345, and from the large air receiver placed at the end of 90 feet of 8 inch discharge pipe, giving the pressure line *A-B*. During the discharge of the air from the cylinder, a pronounced drop in pressure is noted along the line 234. That this is little effected by any question of valve opening is shown by the actual pressures at the same time in the adjacent pipe line shown in dotted lines. The pressure in the receiver *A-B* did not vary an appreciable amount, as shown by the card, so that we have the condition from 3 to 4 of air flowing from the cylinder through a pipe line against an apparently higher pressure in the receiver of over four pounds.

29 The energy of the moving air column obtained from the excessive work done in starting that column from 1 to 3 is being partly

restored from 3 to 4 and is sufficient to overcome the excess of pressure in the air receiver. The elastic variation of these pressures may have such a phase with the speed of the machine as to produce quite different effects in the two ends of the same air cylinder. In compressors having restricted inlet valves these increased pressures due to inertia at the beginning of the compression stroke are not obtained in the cylinder, although they can be found in the ports leading to the valves. The foregoing evidence is such, I hope, as to remove the suspicion that a card showing a raised pressure at the moment of beginning compression is, therefore, remarkable.

30 The third cause of leakage, however, is responsible for most of the cases of excessive pressure at the moment of compression, and I have cards illustrating this from all types of valves which have come to my notice. In each case where leakage of the discharge valve raises the initial pressures, contributory evidence is found of the same fault of leakage in the expansion of air in the clearance volume following farther in the stroke than should be expected. Evidence of this can be found by a careful study of Fig. 17. The raised toe of the diagrams Fig. 17 is probably due to inertia and low resistance through inlet valves and in one case to leakage.

31 The volumetric efficiency of the compressor as a whole is the mean of that found for the four cylinder ends. In Fig. 17, the mean volume of the four cards at any given pressure was taken in forming the combined composite card of Fig. 16. The volumetric efficiency was taken as $AB \div HB$ in Fig. 16. I am unable to agree with Mr. Saunders' computation of the clearance volume expansion. In the adiabatic expansion of air from 34.26 pounds to 13.96 pounds the volume would increase 88 per cent, and 88 per cent of the clearance (2.61 per cent) is 2.3 per cent of the piston displacement instead of the 6.39 per cent which he figures. This would give a volumetric efficiency of 97.7 per cent. The well jacketed heads could be expected to reduce this volume somewhat by cooling, and I have no doubt a small leakage of some inlet valve may contribute to the lessening of the space HA shown on the cards.

32 That the conventional way of measuring the quantity of air delivered by a compressor is far from satisfactory and may be quite inaccurate, is well known, and I heartily agree with the objections raised by Mr. S. A. Moss in his discussion; although I do not believe the loss is usually so large as he implies. The only excuse for continuing such a method is that, whatever it means, the results are expressed in the same terms as all other similar tests, and that there is a lack of familiarity with any more accurate method readily adapted

to the conditions usually surrounding a test of a machine in the field.

33 It was the special object of this work, as stated in paragraph 2, that the test should be "under daily running conditions," and the only change from those conditions was in maintaining a uniform speed which was about the daily average speed. This was done to minimize errors in computing the power from cards, and does not introduce a refinement tending to raise the efficiency an amount sufficient to warrant Mr. Saunders' statement that the work was "under conditions carefully adjusted for test purposes." To this statement I must object, for careful investigation would show several conditions as to adjustment which could be bettered and the showing quite materially raised if the object had been a super-refined condition as implied.

34 Mr. W. Y. Lewis, Jr., in ignoring the thermodynamic facts presented, falls into the same error as Mr. Saunders in considering that the steam consumption rather than the heat consumption is a proper measure of the call of an engine on the coal pile. The fallacy of this has already been shown, and as to whether the saving of 20 per cent of the coal will warrant the increased capital expenditure is a matter that must be settled for each separate case and include more factors than have been considered either in the paper or in the discussion.

35 Mr. Lewis' regret that information on several subjects of interest relating to the plant was not given, must be met by the author's statement that on any test of so large an engine embodying so many interesting features, a large book might be written and it was the intention rather to write a paper, focusing attention on the actual performance and upon the unique features of the feed water heating system and high steam pressure which together are responsible for this new high duty record. That this system is not readily grasped at present by engineers is shown by the discussion, but that a more careful study of its possibilities is warranted is shown by the high duty attained. This principle is not necessarily accompanied by several expansion cylinders, but is applicable as well to compound engines although perhaps not so readily. It would seem to be especially easy to apply this cycle to turbine engines. That the actual running of the engine as a whole, the peculiar heating system in particular, and the use of high pressure steam have been satisfactory and continuous and free from trouble is the report of the operating engineer. It is my opinion that in this plant there has been no trouble with the boilers due to oil, such troubles as have arisen being more easily explained along other lines.

36 Regarding the high volumetric efficiency, reply has already been made, and to the easy way in which Mr. Lewis would "upset the figures given for efficiency" without specific statement of any kind, no answer can or need be made It is not apparent what evidence can be gathered from the paper or the running of the machine to warrant the reference to a balancing problem, and familiarity with the running of the machine does not suggest a lack in this respect.

37 Mr. Lewis would prefer a compound engine because of its superior mechanical efficiency, but looses sight of the fact that the ultimate result based on the delivered work in spite of the greater number of cylinders involved is still about 6 per cent better than the best pumping engine practice, although an air compressor must involve a greater amount of mechanism and friction. To attain this high result, recourse was had to a thermodynamic principle new in practice which involved high pressures to which the compound engine was not suited and to which the quadruple engine was peculiarly fitted for reasons, among others, of its adaptability to the working out of this principle in the heater system. For mechanical reasons the four air cylinders naturally follow the selection of four steam cylinders.

38 The assumption that the boiler pressure used is troublesome is unwarranted. A new boiler has recently been started in this plant under the high pressure with as little difficulty as could attend the cutting in of any new boiler under the more common pressures and with certainly no more trouble than is involved in superheater practice. Mr. Lewis' advice to the builders to devote their attention to "the more simple and practical compound engine," of which they have many examples, must be coupled with the fact that the best high duty pumping engines are quite generally admitted to be triple expansion engines. The actual horse power involved is usually far below that of the compressor under discussion, and there appears no reason why a compound engine should be the best for pumping air, and a triple best for pumping water.

39 The briefness with which some of the results are presented has been criticised, and the author is quite ready to admit that in the process of boiling down what might have been a long paper he has gone too far and eliminated too much. Many points in such a test are of no value to the casual reader but of great interest to one looking for the way in which the matter has been handled before. For the sake of any who may have a similar problem, the whole of the observations were included even at the risk of incurring the just criticism raised by Mr. Barrus. The temperature-entropy diagram

was introduced for the same reason, as it was thought to be of use only to those attempting to solve the same problem, for the form must contain more than the usual entropy diagram to show the abstracted steam and the heating device.

40 The several combined diagrams were made as follows: Cards were taken from each end of each cylinder at the same instant by electrically controlled indicators, and, therefore, represent an instantaneous condition of the use of steam. Of the twenty sets of cards, one typical set was selected (No. 10) which gave a horsepower close to the average and whose cards were as good as had been made. Since the two ends of a cylinder did not use steam in exactly the same way, the cards were drawn in the form of Fig. 14 showing both cards. In this redrawing, the mean effective pressure to the new scale and the mean effective pressure of the original card were compared to act as a check on the accuracy of the work. Since an entropy diagram could not be drawn from a card having such double lines, a new card, Fig. 15, was produced by taking the average pressure of the two as found at each abscissa representing piston position and checking the mean effective pressure of this combined composite card against the average of the two original cards from which it was made. The saturation curves for this diagram were drawn for the weights charged to each cylinder given in P25. To eliminate the effect of the clearance steam, Fig. 13 was drawn by the method given in Sidney Reeves' "Thermodynamics of Heat Engines." From this the entropy diagram, Fig. 18, was constructed. Horizontal distances represent the entropy of the mixture used per minute and if it is borne in mind that the first intermediate cylinder does not have the same weight of steam fed through it as the horse power cylinder, the reason for the "broken water line on the left of the diagram" is at once apparent, and also the broken dotted line to the right which represents the entropy of saturated steam fed through each cylinder. As the weight of steam fed to each succeeding cylinder grows less, the entropy of steam and water is less and moves these lines to the left. The chart is an entropy diagram of the four engines to the same scale, but each using a different quantity of steam, combined with a chart showing the distribution of the heat which is withdrawn between each cylinder and put into the feed water, and a chart showing the disposal of the reheater steam.

41 These later features are unusual and give the diagram an unfamiliar look. From the entropy diagram of each cylinder the steam accounted for at any point in the stroke can be determined by a simple measurement, as asked for by Mr. Barrus. In the upper work

area of this diagram for the horse power cylinder the steam is shown to be gaining heat from 2 to 3.

42 From 3 to 4 there is a loss of entropy during the terminal drop at exhaust and heat is rejected during exhaust from 4 to 5. The weight sent to the next cylinder being less, the reduced entropy of both water and steam is shown by drawing these dotted entropy lines to the left, and the area under the horizontal line just below 4 is proportional to and represents the heat sent to the next jacket and to No. 4 heater.

43 On the left of the diagram, part of this area appears as representing the last increase in temperature and entropy of the feed water. The area representing the heat sent through the first jacket and the reheaters is shown slipped to the right to avoid confusion. The horizontal width of this area represents the entropy sent to the horse power jacket and at the temperature level shown. The area below this top horizontal line represents the total heat sent to this jacket.

44 When the steam carrying the heat enters the first reheater it is found to have dropped to a lower temperature level, as shown, and to have lost entropy and the area below this line represents the heat sent to the first reheater. The difference in the two areas, therefore, must represent heat given up to the horse power cylinder, most of which is shown by the gain in heat in steam falling in temperature from 2 to 3 in the horse power work diagram. Similiarly each loss of heat is shown.

45 The areas (down to an absolute zero line) representing heat lost to feed heaters, check very well with those trapezoidal areas on the left representing heat gained by the feed water as it starts at a low temperature level and has its weight, temperature, and entropy increased until it reaches a final temperature of 334.5 degrees. Had the engine developed a perfect Carnot cycle the entropy of the feed water leaving the engine would have been shown at the intersection of the dotted entropy-of-water line and the highest temperature level. This is left for the economizer to close. How near this theoretical condition is reached is seen at a glance and it is this feature of the engine which is unique.

SAW-TOOTH ROOFS FOR FACTORIES

By Mr. K. C. Richmond, Published in November Proceedings

Mr. Knight C. Richmond I was much interested in Professor Sweet's discussion of Mr. Hinds' paper, as a saw-tooth roof of similar detail to that described by Professor Sweet has been in use for some years at the Port Chester Bolt and Nut Company and was found so satisfactory that the design was copied in extending their machine shop, about seven years ago.

2 Referring to Mr. Green's statement that properly proportioned forced ventilation will obviate the necessity for a high humidity in textile work, and therefore some of the troublesome roof condensation, I think it fair to state that the most serious cases of roof condensation which I have seen have occurred in two weave sheds where a fan system of heating was used. In each case the air was drawn from the basements which were, I confess, somewhat damp, and forced through heating coils and delivered by ducts at various places in the weaving floor. There was no means of regulating the humidity of the basement air, but the roof condensation was in evidence to an unfortunate degree.

3 Mr. Kunhardt misunderstood my meaning when he thought that in furring up the roof, I countenanced leaving any hollow spaces. My use of the word *furring* was perhaps misleading, for I have always used solid filling under the roof covering without concealed air spaces.

4 In spite of Mr. Haight's reasons, I still feel that the greatest efficiency combined with economy is reached by inclining the sash as far as possible from the vertical without introducing troubles from direct sunlight.

5 Mr. Fordyce's interesting saw-tooth roofs meet his conditions well, but our troubles are quite as much in keeping cool in the summer, as warm in winter.

6 I am indebted to Mr. Hinds for his criticism of my statement that there is little argument for saw-tooth roofs in heavy machine shop work. The discussion has shown their advantages in many places in such work, especially when great height is not necessary, and I gladly accept the amendment. My feeling is still in favor of conductors passing down inside the building, rather than pitching the roof for draining the water to the walls. Some bad cases of frozen outside conductors, and damage to roofs from ice forming at the jet, have made me feel that it is more satisfactory, in New England at least, to keep the roof water away from the outside walls.

7 The discussion has brought out so many interesting and valuable points, that I hope it may be continued, and that the perfect saw tooth roof may shortly be evolved from the varied experiences of the different members.

THE ART OF CUTTING METALS

By F. W. Taylor, Published in Mid-November Proceedings

Mr. H. Le Chatelier Few discoveries in the arts have been the occasion of so many successive surprises as those of Mr. Taylor. At the time of the Exposition of 1900 in Paris, nobody quite believed at first in the prodigious result which was claimed by the Bethlehem Works, but we had to accept the evidence of our eyes when we saw enormous chips of steel cut off from a forging by a tool at such high speed that its nose was heated by the friction to a dull red color. The use of the high speed Taylor-White steels spread rapidly in our manufacturing plants; we became accustomed to them and were no longer astounded at their qualities. People did not even take the trouble to investigate the exact origin of this great discovery. The legend sprang up that the invention was made through the carelessness of a workingman, who had accidentally overheated this tool and thus, contrary to all predictions, had greatly improved it. People were satisfied with this explanation, and the originator of the discovery did not even take the trouble to deny it.

2 Some years later, he decided, at a time when people had ceased to speculate on the subject, to set matters aright. The new tool steels had been discovered as a result of an extended series of experiments which had been made with many hundreds of tools, and which had required during long years the permanent coöperation of both educated engineers and workingmen who devoted their time especially to this investigation. The legend had been disposed of. We thought this time surely that we knew the entire truth, but that was not at all so.

3 Mr. Taylor tells us now that the discovery of the high speed tools is merely one single step among a long series of investigations of equal importance; investigations which have been carried on for the past twenty-five years with the coöperation of some ten great industrial establishments, at an expense of nearly $200,000. The inventor informs us with legitimate pride that during all these years none of the results of these experiments had become known to outsiders. The promise of secrecy which had often been given merely verbally, had

not been violated by a single manager or even by a foreman. We cannot too greatly admire such an example of probity in the arts.

4 It is very difficult, on rapidly reading a paper so full of important tables, formulæ, and conclusions, to grasp at once all of its salient features; it will be necessary to study it at leisure. Certain facts, however, will strike any engineer by the clearness with which they are set forth and will be at once adopted as the standard practice in our machine shops; for instance, this first thesis: "It has been found that the greatest economy is attained by running a tool at a cutting speed which will ruin it in one hour and one half." Or, again, the second thesis: "The cutting speed of a tool can be increased by 40 per cent, if its nose is kept wet with a heavy stream of water."

5 These are facts so easily verified that one is justified in being astounded with the author that they are not known to everybody. The simplest discoveries are generally the most difficult ones to make, but they also involve in their application results of the greatest importance. The near future will show us the service which has been rendered to the mechanical arts by the generous publication of researches pursued with such uncommon perseverance.

6 But even now we can admire without reserve the scientific method which has controlled this whole work. It is an example unique in the history of the mechanic arts. We have all admired the researches of Sir Lothian Bell, on blast furnaces, and those of Sir William Siemens on the regenerative furnace; but notwithstanding the high scientific value of the work of these two great engineers, on reading their papers neither of them leaves an impression on the mind which can be compared with that of Mr. Taylor's paper. It is a model which every young engineer will have to study.

7 We believers in the scientific study of the arts have all been teaching our students that in investigating practical problems it is necessary, first, to accurately determine and define the various factors which enter into the problem; then to classify these factors according to the degree of their importance, and lastly, to study the effect of each of these factors as variables independently. But when we see how little respect is shown to these principles even in the laboratories of pure science; when we see the sovereign contempt with which they are treated by every technical man, we come sometimes to doubt our own teaching, and to ask ourselves whether after all we are not teaching our young men wrong methods.

8 And yet, the systematic application of these very methods—nobody can deny it any longer—has been preparing during all these years to transform machine shop practice from the domain of rule of thumb

to that of exact science. The scientific method is now about to receive the crown which it deserves, thanks to the generous publication of the President of The American Society of Mechanical Engineers.

9 Among the varied subjects investigated by Mr. Taylor, the author of these remarks can select only one on which he can give a competent opinion. It relates to the chemical composition and to the thermic treatment of the high speed steels.

10 One of the essential points is the determination of the most suitable chemical composition for the new steels; the data of the original patent which are reproduced in the first column of the following table, showed too high a percentage of carbon, and too low a percentage in chrome and in tungsten.

	Taylor Patents[1] 1900	Le Chatelier[2] 1904	Gledhill[3] 1904	Taylor 1906
Carbon	2	0.5	0.55	0.67
Chromium	1	3	3.5	5.6
Tungsten	4	12	13.5	18
Vanadium	0	0	0	0.30

11 In 1904, after a comparative study of the best brands which at that time were to be found in the market, the composition of the second column of the table was recommended by me. Some months later, Mr. Gledhill published an almost identical composition, which was the result of experiments which he had made at the Armstrong-Whitworth works.

12 The composition recommended by Mr. Taylor in his presidential address differs notably from the preceding ones; the percentages of carbon, of chromium, and of tungsten are nearly 50 per cent higher, but the most important new point concerns the addition of a small quantity of vanadium and the absolute exclusion of molybdenum, which latter had, on the contrary, been considered by me as being rather superior to tungsten. Experiments made on a small scale did not enable us to recognize the irregularities which were due to the presence of this metal.

13 The difference between the hardness of the tool when cold and the hardness when hot is perhaps not so great as the author of the paper seems to claim; that the high speed steels are generally less hard than carbon steels suitably treated, is a fact recognized by every-

[1] French patent of June 12, 1900. Revue de Métallurgie, 1 bis, 187. 1904.

[2] Revue de Métallurgie, 1, 341. 1904.

[3] Iron and Steel Inst. Meeting, New York, Oct., 1904, and Revue de Métallurgie, 2 bis, 113. 1905.

one. It has already been demonstrated[1] that it is possible to make a clearly visible impression on the high speed tools, with a Brinell tool—something which is impossible on ordinary tools. The important characteristic in which the high speed tools and the carbon tools differ from one another is that the temperatures at which they lose their hardness differ widely; it varies from 300 degrees for carbon tools to 700 degrees for high speed tools. The high speed tool, which is the least hard while cold retains its hardness almost unimpaired at a dull red heat. These facts seem to be readily explained, as Mr. Osmond has shown, if we admit that the presence of chromium and tungsten increases still further the passive resistance to the transformation of the carbon on which, exclusively, the phenomena of annealing depends.

14 Mr. Taylor's observation regarding the absence of any relation between the quality of a high speed tool and its micrographical structure or the quantitative degree of its divers physical properties which can be measured, has nothing surprising in it. The same is true of carbon steels. If you compared two samples of one of these last mentioned steels, one heated to 780 degrees and cooled to 200 degrees, the other heated to 900 degrees and cooled to 250 degrees, the first will make an excellent tool, the second will be frightful. Nevertheless, their microscopic structure, and their electrical conductivity will be identical.[2]

15 We must not conclude, however, that the investigation of the physico-chemical properties of tool steels is useless. Such investigations can without doubt give necessary characteristics in steels for making tools of good quality although not all of the characteristics which are necessary for this purpose. Thus, carbon steel with 1.13 of carbon should show a structure which lies between that of martensite and troostite, and have a resistance equal to 1.4 times that of the annealed steel. These conditions are necessary in good tools: Steel which consists entirely of martensite, with an electrical resistance near 2 would certainly be too brittle, and unsuitable for use; steel consisting entirely of troostite, with resistance near 1, would certainly be too soft; a structure between martensite and troostite and a resistance 1.4 times that of annealed steel is certainly a characteristic of the best tool, but the reciprocal will not be true, these conditions alone are not sufficient to indicate with certainty a first-class tool.

16 It will doubtless be possible to find in the same manner for high speed tools, conditions of like character which are essential; but it is

[1] Revue de Métallurgie, 1, 342. 1904.
[2] Revue de Métallurgie, 1, 186. 1904.

very doubtful, however desirable it may be, whether we shall be able to find all of the conditions essential for the best high speed tool.

17 The knowledge even of some of the characteristics necessary for a first-class high speed tool would be of great value. This would permit us to reduce the number of tests and to eliminate *a priori* such alloys as are without doubt useless. The great expense of making the experiments which are now necessary to test the efficiency of tool steels, renders any process valuable which would permit us to restrict their importance.

18 It seems as though it would be particularly desirable to make additional investigations in the direction of measuring the electric resistance and the magnetic properties of the best high speed tools.

MR. C. CODRON We have read with the greatest interest Mr. Taylor's paper, describing his remarkable investigations on the art of cutting metals. There can be nothing but praise for this work, which records the results of an enormous number of experiments, involving an expenditure of time, money, and talent which has no equal in the annals of experimental investigation. It is not possible for us to comment hastily on a work of such importance. I can, nevertheless, say that Mr. Taylor has investigated the subject of turning metals from all practical points of view, in a most careful and thorough manner. He has brought together the tests of Sellers and Nicolson, arranging them for comparison with his own, neatly and precisely, in tabular form, so that many points are cleared up which heretofore have not been well understood. It seems that no element connected with cutting metals in a lathe had been neglected by him.

2 Although he limits his investigations to the turning tool, he shows by the great extent of his work, by the various elements to which he calls attention, and by the interesting discussion which they bring forth, the great complexity which a simple cutting tool presents when working upon iron and steel. He shows likewise all the difficulties involved in carrying out comparative tests in this field when endeavoring to make them, first under uniform conditions and then under varied conditions.

3 Those who are not familiar with work of this nature must be greatly surprised that a subject apparently so small is capable of such great development. We must congratulate Mr. Taylor on having included in his paper so many wise conclusions and comments, showing wonderful ability as a trained and accurate observer.

4 We will make only one remark, which relates to diagram on Folder 12, Figs. 82 and 86.

According to the tests of Mr. Nicolson, the pressure on the tool decreases when the speed increases. At first very rapidly, then very slowly.

Now, our experiments with cutting tools have led us to the opposite conclusion, and we have also reached a similar conclusion for punching and drilling.

5 We maintain *a priori* that the same law exists for all different metals; if, for example, we experiment in cutting a metal which is very soft and very ductile, such for instance as lead, in a metal cutting machine which allows us to use very low speeds, say only a few milli-

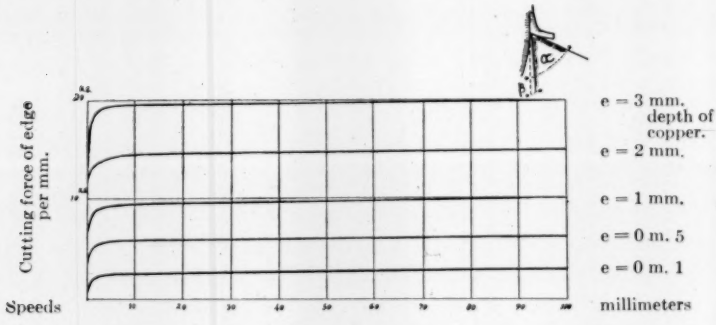


FIG. 1 CUTTING LEAD AT A VARIABLE SPEED, $\alpha = 75°$, $\beta = 5°$

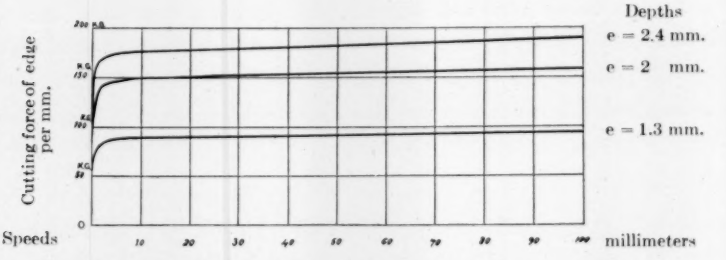


FIG. 2 CUTTING COPPER AT A VARIABLE SPEED, $\alpha = 45°$, $\beta = 5°$

meters in several hours, we find a law clearly indicating that the pressure increases rather rapidly with an increase of speed at very slow speeds, and that it then tends to remain constant or to rise little. With hard metals, such as iron, we have been unable to deduce any law, because the cutting tool causes the chip to break off suddenly, and consequently great variations in the pressure are recorded. In cutting hard steels, also, the chip is broken up into sections, thus producing great variations in the pressure. This, by the way, is also shown in Folder 12, Figs. 82 and 86, of Mr. Taylor's paper.

6 Since the speed in turning metals of this character is always relatively high, we can therefore neglect the slight effect which the speed has upon the pressure at ordinary speeds. We have arrived at similar conclusions in our experiments in shearing, punching, and drilling metals.

7 We state with pleasure that the curves which Mr. Taylor shows as representing the average of his many experiments agree in their general outline with those of our tests, which we have not yet published, and which refer to cutting tools in general.

8 We are sorry that we cannot speak more at length of the paper. Mr. Taylor has had the kindness to highly praise our experiments on drilling; for this we thank him most cordially.

9 The two accompanying diagrams represent the varying pressures on a lathe tool in cutting lead and copper at varying speeds, and with various thicknesses of shaving.

MR. WILFRED LEWIS I have read with much interest Mr. Taylor's address "On the Art of Cutting Metals," and wish to express my appreciation, not only of its great scientific value, but also of its far reaching importance to the metal industries of the whole world; and I venture to predict that as time goes on, this art, which Mr. Taylor more than any one else has helped to put upon a scientific basis, will grow vigorously, resting upon the solid foundation he has laid.

2 Hitherto the fundamental questions as to cutting tool, speed, and feed have been left to the judgment of the mechanics or handymen running machines, and the employer of labor has been practically adrift at sea, but with the knowledge of what Mr. Taylor and his associates have done before him, he is now in a position to demand a return in product commensurate with his facilities for production, and to offer a liberal compensation to labor for the performance of the task assigned to it. He is no longer obliged to feel his way in the dark, and accept with thanks the crumbs of comfort that may come to him as the result of tempting offers to increase production, not knowing what should or should not be expected in any case. But overwhelming as this paper is in the mass of detail covered, and the labor involved in the fruitful generalizations therefrom, the important fact should not be overlooked that it is only a part, and a comparatively small part, of a much larger subject which was presented to this Society by Mr. Taylor more than three years ago.

3 It has been my good fortune to have had some experience with the Taylor system of shop management, as outlined in that paper, and I believe it is only through this system of mechanical management

that the full benefits of his work "On the Art of Cutting Metals" can be realized.

4 The success of this system is made to depend upon the success of all concerned, and the benefits of increased production are shared by the men who contribute to them in the following explicit instructions, as well as the leaders who fix the tasks to be performed. Obviously, the larger the plant the greater the need of organization, and the larger the results to be expected from any system of management; so, having but a small plant, with only 140 employees, including office, business, and selling force, I was at first in doubt as to whether the Taylor system could be established successfully.

5 As with any new piece of machinery, the expense of installation is a heavy burden until it begins to pay, and for the first two years progress was impeded and almost stopped by the obstructionists who knew "something better," but during the last year, the intrinsic merit in the system has told plainly in the results produced, and with a slight reduction in the number of our machinists and a large increase in our managing force, resulting in a total working force of about 160 men, as against the former force of 140 men, we have more than doubled the production of the previous year and converted a loss of 20 per cent on the smaller volume of business into a gain of 20 per cent on the larger volume. At the same time, our working force has become united, contented, and happy in the better wages which we have been able to pay for strict attention to duty and satisfactory performance of the allotted tasks. Better times, better sales, and the marketing of superior products have also helped to bring about better results, but the increased capacity of our plant under the Taylor system of management is the important fact to be noted. This has been doubled and may possibly be doubled again as the system now established becomes more perfectly adapted to our needs. Its general principles are not affected by the character of the business to which they may be applied, but in introducing the system, the concrete forms in which those principles are cast demand the constant attention and skill of an expert in their preparation.

6 I would, therefore, call particular attention to the fact that it is not desirable and hardly ever even practicable for the managers of a company to introduce the Taylor system themselves; that is, to undertake its introduction without the help of men who have been especially trained to this work. It is exceedingly difficult for anyone who has not had personal experience in meeting and overcoming the obstacles that are sure to be encountered, to introduce it and avoid, while so doing, the danger of strikes, and also of possible deterioration in the quality and quantity of the output of the shop.

7 During the period of introduction of the Taylor system, the entire attention of the management is especially needed in the actual daily running of the works and business and, as Mr. Taylor has said in his paper, "Any company is indeed fortunate which can secure the services of a man who is competent to introduce it." I feel that the Tabor Manufacturing Company has been especially fortunate in this particular, and that our successful introduction of the Taylor system would still be in the dim and distant future, had we not secured very early in this work the services of a competent expert like Mr. Barth, and also, particularly, his assistant, Mr. H. K. Hathaway, who stood by us and helped us through a very trying and disheartening period of development. Now that we are fairly on our feet and beginning to run along without Mr. Hathaway's assistance, we realize that when once introduced, it is far easier to run our works on the Taylor system than by any other system of management.

8 It is not possible to present an adequate discussion of Mr. Taylor's paper "On the Art of Cutting Metals" at this time, and not for some years perhaps will its real value be understood and appreciated, and back of it all will then begin to grow the larger subject of which it is only a part, the labor problem reduced to its simplest terms by unlimited production and corresponding remuneration for the men who want to work hard and get paid for what they do.

Mr. R. A. Skeggs Owing to the limited time before the meeting we have been unable to go very thoroughly into Mr. Taylor's work "On the Art of Cutting Metals." We can, however, say that the conclusions arrived at by Mr. Taylor confirm generally our own experiments and experience.

Taking them up by paragraphs:

2 (Par. 83.) The speed given for $\frac{1}{8}''$ cut x $\frac{1}{8}''$ feed is 10 feet per minute and that for $\frac{1}{2}''$ cut x $\frac{1}{32}''$ feed is $16\frac{1}{2}$ feet. We contend that, provided the work was rigid, if the tool were ground in such a manner as to cut off chips exactly similar in form, the speed would be the same in both cases.

3 (Par. 148.) We have frequently observed that tools will cool down and appear to cut with greater ease after running for some considerable time. We have accounted for this by: (*a*) A change in the hardness of material. In some cases forgings have been unevenly chilled to such an extent that the cuttings from one side have come off blue, while from the other side they have been almost colorless. (*b*) The tool lip surface becoming worn in such a manner as to give less resistance to the chip and consequently less friction. (*c*) It has

been observed that a tool, running on an annealed forging of regular toughness so far as can be detected, which has been removing blue chips will cut off chips of a lighter color as the diameter is reduced, the speed of course remaining constant.

DIAMETER

4 Our experience is that the standard speed varies with the diameter. For example the standard speed would be higher for a 6 inch diameter than it would be for a 12 inch diameter, and it would be interesting to know whether Mr. Taylor has made any experiments in this direction. We suggest that the reason for this is that the material on large diameters approaches the tool at a steeper vertical angle, the chips having more support behind it, so that with very large diameters the conditions of cutting more nearly approach that obtained with planing. There does not appear to be any reference to the height of the tool with relation to the axis of the work, whether the cutting edge is on or above the center, or the relation of the tool angles to the diameter when making experimental tests.

5 (Par. 352 B.) The section of roughing tools is given as depth $1\frac{1}{2}$ times width. If, as stated in 509, the feed gear should give a pressure at the cutting edge equal to the pressure of the chip on the lip surface of the tool, should not the section of tool be square? Our experience in machining heavy forgings has led us to use a coarse feed and a square tool.

6 (Par. 585.) Chatter of the tool necessitates cutting speeds from 10 to 15 feet per minute slower than those taken without chatter. Has Mr. Taylor any formulae for obtaining working speeds when scale, uneven hardness, and very occasionally sand are to be met with in the forging?

7 Our experiments confirm generally the conclusions derived from the use of alloys which are dealt with under the heading of "Chemical Composition and Heat Treatment of Tool Steel."

DR. J. T. NICOLSON I was pleased to receive by the courtesy of the author, an advance copy of the Presidential Address to this Society on the art of cutting metals; as also a request both from the Secretary and him personally for a contribution to its discussion. It proved a formidable task to read and digest even those parts of the paper in which I was personally more interested; and the portions referring to the composition and heat treatment of the steel I have had to pass altogether by.

2 I must preface my remarks by thanking the author for the gift of the great amount of information contained in his paper. I think all engineers and especially those concerned with the practical side of the art of cutting metals owe a debt of gratitude to Mr. Taylor and the men associated with him, for the pioneer discoveries they have made in connection with tool steel.

3 In reading this paper, which gives an account of these discoveries, as well as of other work in this art, one receives the impression of long continued and well-directed effort pursued with one main purpose. This purpose I take to be *the examination of the various elements which contribute to economy in the rough-cutting of steel and cast iron;* in the process, *i. e.*, of reducing the sizes of forgings and castings from the rough state to that on which the finishing cuts can be taken. The importance of such examination is evident, if as appears to be the case, it be more economical to forge roughly and turn off the excess, rather than to forge so closely to size as to require only a finishing cut. These elements may, broadly stated, be set down as three in number:

a The cutting power of the tool steel as dependent on its own nature;

b The cutting power as dependent on how the tool is used; as, *e. g.*, whether with a high cutting speed and light cut, or with a lower cutting speed with a heavy cut; whether with a keen or a blunt angle and a rounded or straight contour of the cutting edge; and as to what proportion the time spent in actual cutting should bear to the time required for grinding and dressing the tool.

c The improvement of the machine tool; so that, for example, its driving and transmission mechanism shall be so designed and constructed as to waste the smallest proportion of the mechanical energy supplied, and the minimum fraction of the machinist's time.

4 As regards *a;* the whole world is agreed as to the enormous advances the introduction of the Taylor-White heat treatment of steels of a certain composition has effected in the art of cutting metals.

5 As to the above described element *b*, I am by no means disposed to concede all the credit which the author, in various parts of the paper, appears to claim, in reference to improvements in the mode of using the high heat steel of which he speaks.

6 And as to element *c*, in regard to which a knowledge of the laws of the cutting pressures are of at least equal importance to that of the

laws of tool durability, although the author appears himself to have spent a great deal of time on the matter, it must have been quite without the consent of his better judgment; for in Par. 489 he writes: "Our principal object in writing at such length on this subject (*i. e.*, pressure on the chip) has been to endeavor to make it clear that . . . . a study of this element in the art of cutting metals is comparatively barren of practical results."

7 I think it must be admitted that the art of cutting metals includes a great many more things than the author has alluded to in his paper, voluminous though it be. The design and construction of machine tools is just as important a part of this art as the study of tool durability; and as the author has barely referred to the machine tool at all, it is clear that the title of the paper is more extensive in its scope than is warranted by its contents. Apart from this, the title is still too wide; for no reference is made to drilling, slotting, planing, boring, trepanning, milling, sawing, filing, chipping and many other modes of cutting metal of fully as great scope in practice as turning; to part of which field only the author has addressed himself.

8 Descending from the general to the particular; there are a number of statements made by the author in Part 1 (which I take to be a summary of the whole) to which I strongly object; but I think they can be better disposed of by dealing with the more detailed remarks of Parts 2 and 3.

9 In Part 2 there is much matter in my opinion requiring either revision or exclusion; and there are several statements likely to mislead the uninitiated. In respect of personal matters, it appears to me that, notwithstanding the not infrequent words of appreciation and even of approval by the author of the work of other experimenters, the impression produced upon the mind of even the careful student, and much more on that of the cursory reader, will be that the Manchester experimenters, and especially the present writer, have fallen into lamentable and appalling blunders, which have led them to results entirely at variance with those obtained by the more careful and scientific methods of the author.

10 As an example I may cite Sec. 291 in which the author emphasizes most strongly that the most important subject for experiment in this art is the effect upon the cutting speed of *the thickness of the shaving;* and that it is more important to consider this than the area of the cut as a whole, in determining the proper cutting speed of the tool. He goes so far as to say (Part 1, Par. 85) that a formula for the cutting speed *based only upon the area of the cut* is wrong and mis-

leading and that such a formula established on this basis by the Manchester Committee is *valueless* from a scientific standpoint.

11 Now what are the facts? A careful study of the author's results as given in his Tables 45, Folder 8 and 76, Folder 11 (which tables he cites as examples of the most successful series of experiments he has made) leads to the conclusion that, so far from revealing a glaring discrepancy from the law of cutting speed and area, enunciated by the Manchester Committee, the author's results (so far as he publishes them) *do not differ from that law by more than 1 or 2 per cent on the average* throughout the range of depths of cut and

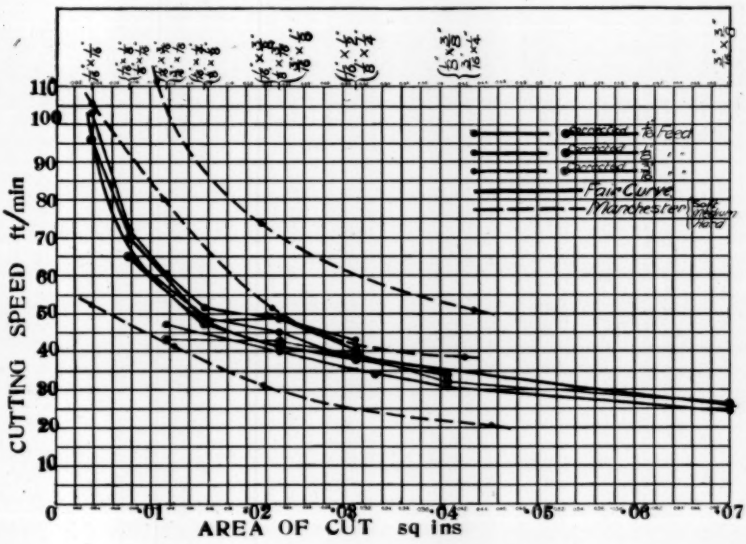


FIG. 1 RESULTS FROM TABLE 70, STEEL

widths of feed ordinarily used for roughing out. The author has given no experimental figures either in Tables 45, 76 or elsewhere to substantiate the statement made in Sec. 281 C that the cutting speed may be $3\frac{1}{2}$ times as great when the shaving is $\frac{1}{64}$ of an inch thick as it is when the shaving is $\frac{3}{16}$ of an inch thick. The finest feed in Table 70 is $\frac{1}{16}$ of an inch. To elucidate the matter I have prepared two wall diagrams, Figs. 1 and 2, from Mr. Taylor's Tables 45 and 76. I have plotted the "standard" cutting speeds as given by the author (and their corrected values as they should, in my opinion, be taken

from Mr. Taylor's own results) upon a base of area of cut; one curve for each width of feed.

12 I have then drawn a fair curve through all the spots on the diagram. An inspection of the way the author's results lie about and around this curve gives good ground for saying that Mr. Taylor's figures substantiate in a remarkable manner the fundamental law that the standard cutting speed with high heat steel for a given material depends only upon the area of the cut and hardly at all upon

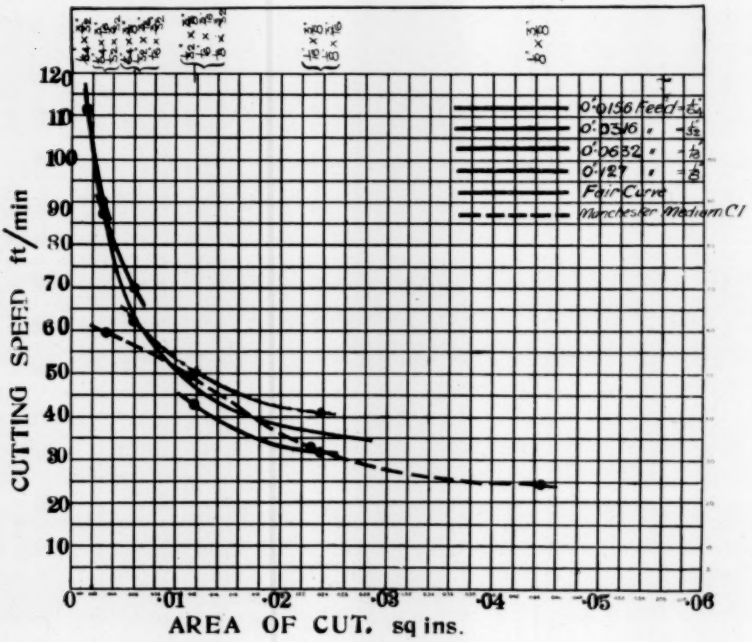


FIG. 2 RESULTS FROM TABLE 76, CAST IRON

the depth of that cut or the width of feed. The author has tabulated no results which show a serious divergence from this law; and until he produces something tangible in this direction the statement he has made in Sec. 281 C must be characterized as entirely misleading. If the author had plotted his results on a base of areas, instead of as contour lines of constant feed upon a base of depth of cut, he might not have been led to make statements of this kind, so patently contradicted by the figures he himself gives.

13 As a matter of fact, a simple formula of the form

$$V = \frac{K}{A + Bc^n}$$

can be obtained to cover, not only the Manchester and Berlin, but also Mr. Taylor's results; with a divergence of only ± 5 per cent.

14 This formula involves only the area a of the cut in square inches, the amount of carbon c in the steel forging expressed as a percentage, and the constants K (depending on the tool steel), B and ^{n} (depending on the other chemical constitutents of the bar); *and makes no reference to either the thickness of shaving or depth of cut.*

15 It may not perhaps possess the accuracy of the author's formula in representing his own results (*vide* Par. 770), of this I have made no trial; but I have hopes of its greater practical usefulness as involving eight fewer constants, and only one variable for a given material.

16 If the author is in possession of experimentally obtained figures, proving that there is a considerable divergence from the speed on area law for such fine feeds as $\frac{1}{32}$ and $\frac{1}{64}$ of an inch, it may still be pointed out that he has himself expressly characterized (*vide* Sec. 252) such light cuts as *worthless* for the determination of the laws of cutting speed.

17 I am prepared to go a step further even than this; upon re-examination of the author's paper in its final form, so far at least as refers to the effect of the thickness of the shaving on the cutting speed (see Par. 291 to 306).

18 In Par. 297, the author gives the formula

$$V = \frac{1.54}{t^{\frac{3}{4}}}$$

for the standard speed of cutting (V), as dependent upon the thickness of the shaving (t). It appears, however (*vide* Folder 16), that the shaving in the experiments, upon which this law is based were all 1 inch wide.

19 The thickness of the shaving in inches is therefore the same as its area in square inches; so that the above formula might equally correctly have been written

$$V = \frac{1.54}{a^{\frac{3}{4}}}$$

where a is the area of the shaving or the product of its breadth by its thickness. We have, therefore, no evidence from these experi-

ments that the increase of the cutting speed as the shaving got thinner was due to the thinning.

20 It may just as logically be said that it was due to the diminution of the area. Similar remarks apply to the formula of Par. 302 in which, with equal propriety, the area might have been substituted for the length of the shaving.

21 In Par. 305 the author seeks to draw the inference that a shaving 1 inch wide by 0.01 inch thick would allow of a cutting speed (1.8 divided by 1.27 =) 1.42 times as great as a shaving of $\frac{1}{3}$ inch and a thickness of 0.03 of an inch. This may hold for carbon steel tools although the conclusion is not to be clearly drawn from the author's experiments even for these. *The wide difference in the chemical composition of the bars used* for the two sets of experiments (Fig. 111, Folder 16, and Fig. 116, Folder 17) renders it futile to deduce the correct value of the effect of proportion of width to thickness in altering the cutting speed for cuts of the same area. The author here in fact commits a breach of his own laws of experimenting.

22 The author adduces no experimental evidence that any similar law holds for *high speed steel tools.* The very different ways in which the carbon steel tool and the high heat steel tool fail, render it extremely unlikely that the standard cutting speeds for shavings of different shapes will vary according to the same laws for both brands.

23 I do not deny that there is a difference in favor of the wide thin shaving when the feed is so fine as to be of the order of 1/100 inch, but I claim that the Manchester Committee's law that the cutting speed depends on the area alone is to a first approximation, true over the whole range of cuts and feeds which are usual in practice, and that its superior simplicity makes it a more generally serviceable rule for the practical man.

24 As another example of the author's method of combining unstinted praise with unspairing condemnation of the work of others, we may refer to Secs. 82 and 83 of Part 1, where, although expressing himself as satisfied that the Manchester Committee's results are of very great practical value, he yet states they were "carried on in a thoroughly unscientific manner."

Mr. Taylor says these experiments were defective because:

a The shape and cutting angles of the tools varied.

b The quality of the tool steel varied.

c The treatment of the tool varied.

d The depth of the cut varied from that aimed at.

e The experiments were not extensive enough.

f The uniformity of the bars from the center to circumference was doubtful.

25 The unsubstantial nature of these serious criticisms will, however, appear upon a closer examination; taking the objections in numerical order:

a It is true that the shapes of the various tools supplied was different for each maker; but with one exception they were all round nosed tools and the difference in the resulting law of cutting speed could therefore not be very great. As to the "lip angles" which varied in the Manchester experiments from 66 to 85 degrees the author himself states (Sec. 265), that the lip angle of the tool, if it exceeds 68 degrees, "makes little if any difference in the cutting speed, when cutting medium hard steels." This fact the author states he has found to be proved by repeated experiments made by himself.

b and *c* Undoubtedly the quality and treatment of the steel supplied by the different makers varied.

26 The object of the Manchester Committee was, however, to ascertain the then existent state of the art as exemplified by a selection of the best known brands of British (and one German) steel. But it appears to me that the author's objection to the Manchester experiments on this ground is nullified by the fact that (v. Manch. Rep. Plates 3 and 4) the cutting speeds as deduced from the mean of all non-failing tools (in 20 minutes) so nearly agree with those obtained from the best in each group. For the purpose of determining "standard cutting speeds" as understood by the author, the various steels used may be regarded as of one uniform brand.

27 *d*, *e* and *f* These objections are also either negatived or answered by a careful study of the Manchester Report, if at the same time the objects aimed at, as well as the methods employed to obtain them, are kept in view.

28 The most complete answer to these criticisms of the author is, however, the fact that the results obtained in Manchester by these alleged unscientific methods, fall absolutely into line with his own, when due allowance is made for the different carbon contents of the British and American bars. This is clear from an inspection of diagrams 1 and 2, and affords indeed, to me at least, one of the best evidences that Mr. Taylor's experiments were carefully made and worthy of general acceptation.

29 The author lays great stress (*vide* Part 1, Secs. 63 and 64, and Part 2, Secs. 144-149) upon his method of determining the variation of the cutting speed as depending upon shape and angle of tool

and shape of cut by the adoption of the 20 minute standard; and seeks to imply that no other experimenters have used it.

30 Now this is the very standard arrived at by the Manchester Committee without any knowledge of Mr. Taylor's work, at that time unpublished.

31 The credit for the fixing of this standard by the Manchester Committee is entirely due to one of its members, Mr. Daniel Adamson. After carefully studying the question, and looking into all the elements of the problem, such as material available, time at disposal, probable cutting speeds, and average durability of tools, he recommended the 20 minute limit to the Committee.

32 It was adopted by them at their meeting on April 21, 1902, and issued in their schedule of Conditions to the Steel Makers on April 23, 1902 (*vide* Manchester Report, p. 232).

33 In view of this fact, the remark made by the author (in Sec. 144) that the accuracy and delicacy of this standard had been but vaguely recognized, would appear to require revision. With regard to my own subsequent experiments on the durability of the tool, as depending on the lip angle, which Mr. Taylor properly, if somewhat severely criticises (Secs. 116 and 117 of Part 2), I beg to be allowed to insert the following quotations from my paper on the lathe tool dynamometer; from which it will be apparent that however much I might have wished to continue the application of Mr. Daniel Adamson's 20-minute standard, it was impossible for me to contemplate doing so, with only the remnants of the Manchester Committee's bars at my disposal for this purpose.

[Extract A from "The Engineer," April 14, 1905]

FORMATION OF CHIP

In order to observe at leisure the action of chip formation, it was arranged to take a cut at a very slow speed. The lathe was driven by taking several turns of a wire rope round one of the cone pulleys and then leading it to a hand winch. When the laborer turned the winch handle at ordinary speed, the cutting speed would be 1 foot in from one to five hours, according to the number of back gears in use. Lines having been ruled on the uncut surfaces of the work, forming squares of $\frac{1}{10}$ inch side, the distortion of these squares during and after the cut could be easily studied.

A chip formed by taking a cut $\frac{1}{8}$ inch deep from a collar $\frac{1}{2}$ inch wide, with a tool square-nosed in plan and having a 60 degree cutting angle, is depicted in Fig. 1. The originally circumferential and radial scribed lines are also seen, and the forms these lines take in a medium hard steel chip have been drawn to a scale about seven times full size.

At such dead-slow speeds the shaving breaks off in a succession of chips, which do not at all adhere to each other, by shearing off at an inclination of about 15

degrees to the vertical along $A\ B$ or $A1\ B1$. This angle does not vary with the angle of the tool, but the angle $B1\ A1\ C$ of the chip becomes greater or smaller according as the tool is keener or more obtuse. The line AB shows where a chip has just been shorn off and has dropped away; and the tool is seen beginning to act upon the wedge-shaped portion $AB\ A1$. However sharp the tool may be,

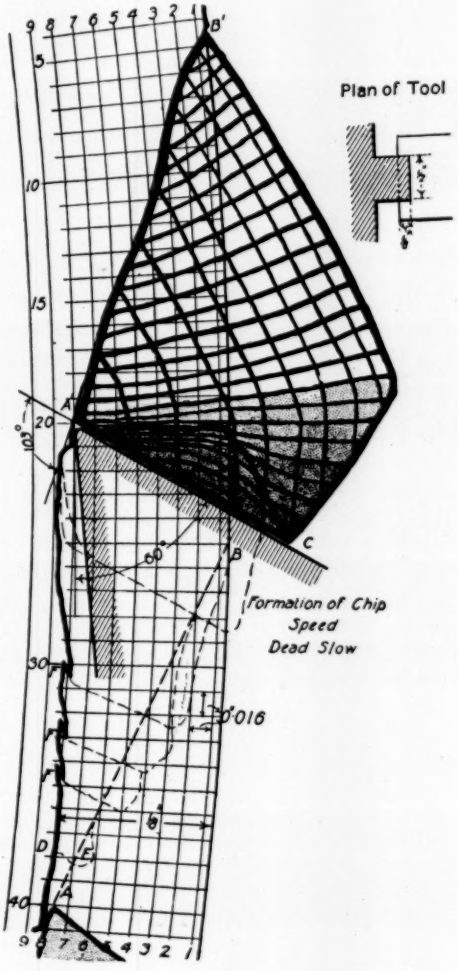


FIG. 1 FORMATION OF CHIP, SLOW SPEED

its edge is still somewhat rounded; and it is, therefore, difficult for it at first to get below the surface of the work. Thus we find that the "surfacing" force—or the horizontal force pushing the tool forward square to the lathe bed—is greater at this time than at any other, although the vertical force is then least, as the last chip has just been cleared off. If the surface of the bar is examined after-

ward, it is found to have a burnished appearance, just at this part, *AD*, where the tool pressed hard upon it. Once the tool enters below the surface it crushes up the material in front of it, and this is caused to flow outwards both along and across the tool surface, as at *DE*. Slipping of this material soon takes place over the tool and away from the work; but this slipping is not continuous, taking place intermittently and at longer intervals the more the tool cuts into the wedge *A B A*1. Each slip is accompanied by a small tear or crack running in front of the cutting edge, as seen at *F*; but these cracks extend for but a short distance at first, being healed up by the radical compression produced by the friction of the crushed material on the top of the tool when it comes to rest after having made a "slip." The same action is seen in the crushing of a short ductile speci-

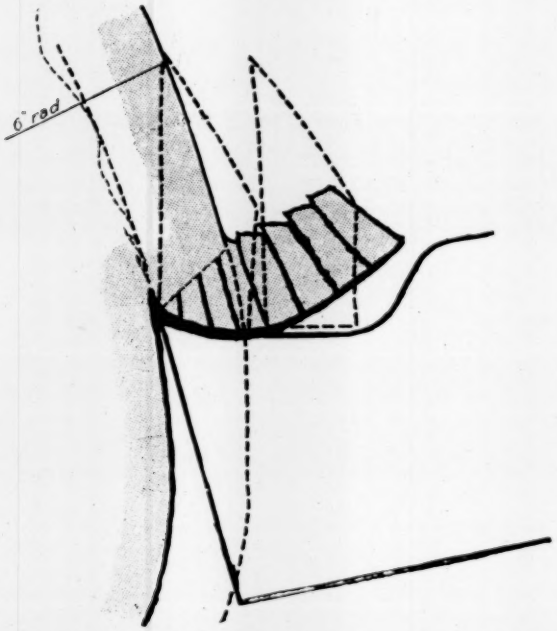


FIG. 2 FORMATION OF CHIP, HIGH SPEED

men in the testing machine; the friction between test piece and platen being powerful enough to prevent cross-extension, uniform throughout its length, from taking place; so giving the crushed piece the barreled shape commonly observed.

These little cracks—*FF*—run deeper, although at longer intervals, as the crushed material spreads further back over the top of the tool, and the consequent "slips" become more violent, until one such tear or crack extends right up to *A*1, and so out by *A*1 *B*1, and the chip drops.

The action at high speed is in many respects quite different from what takes place at "dead slow." The successive main shears—or rather, slides—of the shaving take place at shorter space intervals, for the slipping of the shaving over

the tool now takes place continuously, and the cracks are no longer healed up by compression. The tear from one of the early cracks is now continued right out to the surface of the shaving. The progression of this tear across the shaving is not instantaneous, however, and by the time it has reached the upper surface the chip has advanced some distance from the point of the tool.

If we now refer to Fig. 2, which shows how the shaving is formed at high speed, we see that this region of maximum stress extends from near the point to a considerable distance backward on the surface of the tool. The tear at the very point keeps running ahead of the tool, and is due, not to the direct action of that point, but to the twisting of the material from behind the point where the pressure on the tool is greatest. In fact, the point itself is protected from all action of the work upon it by a small heap of the cut material being collected in the little space opened out by the advancing tear, and resting on a shelf of uninjured tool surface extending from the very edge of the tool to a greater or less distance back therefrom. This heap is firmly adherent, as to its base, to the shelf, but as to its upper part, is continually being removed and replenished from material left on the surface of the bar by the irregularity of the advancing tear. The nose of the tool sustaining the heap only cleans up the shaft, but does not cut the main body of the material from it.

The shaving, at that part of it where the crack is just running across, presses hard, as we have seen above, upon the top of the tool, and by reason of the speed with which it runs over the surface and the high temperature which it imparts to it, gradually wears a groove therein. This grooving starts at some distance back from the cutting edge and gradually advances by wearing away the shelf of unworn tool and dimininshing the size of the heap, until, when the groove reaches the cutting edge and the shelf is all gone the tool "fails." Failure is the process—which occurs almost instantaneously, after the groove intersects the nose of the tool and produces a feather-edge there—of blunting and destroying this edge, the depth of cut then dropping considerably from its proper value. The "work of bluntness" is thereby largely increased, generates a correspondingly larger amount of heat right at the edge of the tool, and the cutting power is immediately destroyed.

34 I will only say, without defending the durability standard I was forced to accept, that the lip angles I recommended for shop use were 75 degrees for medium cast iron and 65 degrees for medium hard steel; while Mr. Taylor's results give a lip angle of from 76 degrees at the front to 68 degrees at the side, for the same materials (Sec. 342 A).

35 There are many other debatable points, I should desire to take up if time permitted.

36 With reference to Mr. Taylor's remarks on the interaction of tool and chip which take up his Par. 153 to 169 of Part 2, I think it only right to say that in so far as the author's explanations are, in my opinion, correct, they were anticipated by me in a lecture I gave at the Graduates' Society of the London Institution of Mechanical Engineers, in February, 1905. In proof of this, I enclose an

extract A from "The Engineer" (where some parts of that lecture were published) of April 14, 1905.

37 I regard Mr. Taylor's explanation of the heating of the tool as erroneous, as I have proved in an article published by me in "The Engineer" of June, 1904, that the work done against friction is only about 13 per cent of the whole, and the tool is consequently chiefly heated *by conduction* from that part of the shaving where the maximum twisting is occurring in the chip.

38 This fact leads directly to rational methods of deducing the expected durability of tools, as depending on tool angles and areas of chip.

39 I cannot conclude without saying that the results obtained by the author with regard to the most economical cutting speed, as dependent upon the proportion of time spent in cutting to time spent in redressing and regrinding, are, in my opinion, of the greatest practical value to all engineers.

I look forward to the deeper study of the author's results along this line with a profound sense of their practical importance and economic significance.

MESSRS. ADAMSON AND NICOLSON

MR. FRED. W. TAYLOR We feel especially indebted to Messrs. Adamson and Nicolson for taking the time and trouble to write at such length in criticism of our paper "On the Art of Cutting Metals," the more so as they are both such busy men. Perhaps no two men are better qualified through their large personal experience to criticise a large part of the work of other people in this field.

2 In Par. 37 of Mr. Nicolson's criticism, he states that in the "Engineer" of June, 1904, he pointed out the fact that the heat of the chip is chiefly caused or generated by the deformation which the metal of the shaving undergoes as it is being torn, crushed, and sheared away from the body of the forging. I have no doubt that Mr. Nicolson is correct in saying that more heat is generated through this distortion of the chip than is produced by a friction of the chip against the surface of the tool. In describing the theory upon which tools work and are destroyed, this fact had entirely escaped our attention, and we are indebted to Mr. Nicolson for calling attention to it.

3 He is, however, wrong in assuming that the tool is mainly heated by *conducting* the heat from the chip to the tool. On the contrary, at all times during the operation of cutting, heat is contin-

ually being conducted from the tool to the chip, and this will be clearly evident to anyone who observes closely a modern high speed tool when cutting at its maximum speed. In this case, the nose of the tool, for a considerable distance below the cutting edge, will be seen to have a dull red heat, while, on the other hand, the chip itself, with the exception of a few loose films of metal on the under side of the chip, is never heated to a red color. It is needless, of course, to point out that heat will always flow from the hotter to the colder body, therefore the chip is continually conducting heat away from the tool.

4 The amount of heat developed through bending and distortion in the chip, however, is, as Mr. Nicolson points out, a very important feature in the whole problem, and undoubtedly if this heat were not generated in the chip through its distortion, the chip would then be so much colder that it would carry off the heat from the nose of the tool much more rapidly, and higher cutting speeds could be attained. This heating of the chip through bending or distortion also accounts for the fact that, in order to be most effective, a stream of water should be directed right on to the chip at the point at which it is being cut away from the body of the forging. The water, which removes a certain amount of the heat generated in the chip, enables the chip to carry off more of the heat from the nose of the tool, and so permits an increase of 40 per cent in the cutting speed. The observation and statement of these facts constitutes another important contribution by Mr. Nicolson to the art of cutting metals.

5 In Par. 20 of Mr. Adamson's criticism, and in Mr. Skegg's criticism, these gentlemen take exception to our statement that the body of the tool should be twice as high as it is wide, as illustrated in Figs. 14, 15, and 16 on Folder 2, and referred to by us in Par. 415 to 421. If it frequently occurred that the feeding pressure on the nose of the tool was equal to the driving pressure on the top surface of the tool, then Mr. Adamson would be correct in stating that a square shank is the proper section for a tool. But it is of very rare occurrence, as carefully pointed out by us, that the feeding pressure is equal to the driving pressure. In deciding the feed gearing for a lathe, we have stated that the feeding pressure should be counted upon as equal to the driving pressure, because, if this only occurred once in three or four months, it would be sufficient to smash the feed gears of the machine, unless they were especially designed to meet this heavy pressure.

6 On the other hand, the normal feeding pressure, which occurs perhaps in ninety-nine out of a hundred cases, is only from 15 to

40 per cent of the downward pressure on the tool. The tool itself, then, should be designed to suit these normal conditions, while the feed gears should be designed to withstand the most severe conditions, even although they occur very seldom.

7 I am particularly pleased to note that Mr. Nicolson's observations as to the manner in which the chip is cut or torn from the forging, as described by him in Par. 36, agree so nearly with our own as described in Par. 153, etc. This fact makes both of these observations much more valuable and especially so since in conducting the experiments and in describing them, neither set of experimenters was even aware that the other was working in this field.

8 Answering Mr. Adamson's question (Par. 19) as to whether there is ground for the prejudice which exists in England in favor of using a grindstone for sharpening high speed tools, instead of an emery wheel, I think there is ample ground for this preference. Unless a very heavy stream of water is directed continuously upon the cutting edge of the tool throughout the time that it is being ground upon an emery wheel, the tool is likely to be seriously injured in the grinding. The ordinary grindstone runs so much slower that it is not so likely to overheat the tool in grinding. However if a proper stream of water is used, there is no question that the emery wheel, with grit of proper coarseness, is greatly to be preferred to the grindstone. It will grind six or eight times as fast under the most favorable conditions, and will not injure the tool in the least.

9 Again, answering Par. 24 of Mr. Adamson, the use of tool holders involves inserted tools of high speed steel which are small in their body, and for this reason they contain too small an amount of metal to properly carry away the heat generated by the friction of the chip, and they therefore necessitate much slower cutting speeds. In addition to this, the size of tools inserted in the tool holder is generally too small to permit of the proper curve for the cutting edge. It is our practice, in the shops which we standardize and systematize, to use practically no inserted tool holders. The one exception which we make to this rule is in the case of tools of the largest size, as, for instance, those used in cutting heavy armor plate or the largest size steel forgings. In this case, a very large inserted tool can be used with advantage.

10 Mr. Adamson's objection in Par. 11 and 12, to the use of a "cut gage" as illustrated by us in Folder 9, Fig. 55, can be attributed only to the fact that he has never used one. I must repeat that this is the only means which we know of by which the same depth of cut can be maintained uniformly throughout the experiment, and it is

needless to call attention again to the necessity for maintaining a uniform depth. The use of the micrometer on the feed screw, as suggested by Mr. Adamson in Par. 11, does not enable the operator to obtain the depth of cut which he aims at, nor does it enable him to maintain this depth uniform, when once obtained. In proof of this, I would call attention to the fact that, throughout the experiments published by Mr. Nicolson in the Transactions of our Society, Vol. 25, Paper No. 1035, June, 1904, in which the micrometer apparatus was used, invariably a given depth of cut was aimed at, and a different depth of cut was actually attained in the machine. It is an excellent plan, on the other hand, as suggested by Mr. Adamson, to have a revolution counter attached to the spindle of the lathe by which to check the other measurements made.

11 Answering Mr. Adamson's Par. 25, it has been our custom to set our tools with their cutting edge practically on the level with the center of the lathe.

12 Referring to Mr. Adamson's Par. 26, as to the difference between acid and basic forgings, we do not look upon the chemical analysis of a given forging as sufficient data for determining its cutting properties. The combination of tensile strength and extension, as indicated in our Table 141, Folder 23, and in the paragraphs throughout the paper referring to this, is the best guide to the quality of the metal being cut.

13 In Mr. Adamson's criticism in Par. 28, and also in Mr. Skegg's criticism, Par. 3, these gentlemen have evidently overlooked our chapter on the effect of the quality or hardness of metal upon the cutting speed, beginning with Par. 1141, and particularly to Par. 1166, in which we refer to the effect of scale on cast iron castings upon the cutting speed.

14 In reading the criticisms of both Messrs. Adamson and Nicolson, the impression is given that in writing the paper we have spoken contemptuously, or at least in a slighting way, of the work which they have done. If, in writing our paper, I have given this impression, I wish most sincerely to apologize. I have the highest respect for the work of both of these gentlemen, and am sure that with their special ability and training they would have accomplished much more in the same space of time than we have done. We have clearly stated, in writing the paper, that we worked for fourteen years with false and unscientific standards, and I have not the slightest doubt that the gentlemen in charge of the Manchester experiments, and particularly Messrs. Nicolson and Adamson, would have adopted thoroughly scientific methods in a much shorter time than this. It must be

remembered that the Manchester experiments extended only over a period of less than a year, and it is not to be expected that, even with the special ability of this committee, they should have hit upon thoroughly scientific methods in so short a time. It was my sincere endeavor in writing the paper to make my criticism of the work of other experimenters strictly impersonal. Our only object in offering criticism being that of inducing any future experimenters in this field to adopt the very best, the most modern, and the most scientific methods. A more careful reading of our paper will, I think, make it clear that our criticism throughout was aimed at unscientific methods, whether they were used by us or by other people; and in fact for every word that we have used in the criticism of the work of other experimenters, we have written ten words in severe and frank criticism of our own shortcomings. Many of our sentences bearing upon this matter must have been hastily read or else overlooked by Messrs. Adamson and Nicolson. We would, therefore, call especial attention to the following sample paragraphs as relating to this subject.

Par. 32 The writer has no doubt that many of the discoveries and conclusions which mark the progress of this work have been and are well known to other engineers, and we do not record them with any certainty that we were the first to discover or formulate them, but merely to indicate some of the landmarks in the development of our own experiments, which to us were new and of value. The following is a record of some of our more important steps.

Par. 58 This method is, of course, much quicker than the thoroughly scientific type, and it is largely for this reason, in the opinion of the writer, that almost all of the other experimenters in this field have chosen it. Several of the experiments of this type have proved most valuable and developed much useful information, and it is with hesitancy that the writer criticises the work of any of these experimenters, since he appreciates most keenly the difficulties under which they worked, and is grateful for the information contributed by them to the art. After much consideration, however, he has decided to point out what he believes to be a few errors made by these experimenters, with the same object which he has in indicating our own false steps: namely, that of warning future investigators against similar errors.

Par. 59 Almost the whole course of our experiments is marked by imperfections in our methods, which, as we have realized them, have led us to go again more carefully over the ground previously traveled. These errors may be divided into three principal classes.

Par. 60 (A) The adoption of wrong or inadequate standards for measuring the effect of each of the variables upon the cutting speed.

Par. 61 (B) Failure on our part from various causes to hold all of the variables constant except the one which was being systematically changed in order to study the effect of these changes upon the cutting speed.

Par. 68 It was only after about 14 years' work that we found that the best measure for the value of a tool lay in the exact cutting speed at which it was completely ruined at the end of 20 minutes. In the meantime, we had made one

set of experiments after another as we successively found the errors due to our earlier standards, and realized and remedied the defects in our apparatus and methods; and we have now arrived at the interesting though rather humiliating conclusion that with our present knowledge of methods and apparatus, it would be entirely practicable to obtain through four or five years of experimenting all of the information which we have spent 26 years in getting.

Par. 69 The following are some of the more important errors made by us:

Par. 70 We wasted much time by testing tools for a shorter cutting period than 20 minutes, and then having found that tools which were apparently uniform in all respects gave most erratic results (particularly in cutting steel) when run for a shorter period than 20 minutes; we erred in the other direction by running our tools for periods of 30 or 40 minutes each, and in this way used up in each single experiment so much of the forging that it was impossible to make enough experiments in cutting metal of uniform quality to get conclusive results. We finally settled on a run of 20 minutes as being the best all-round criterion, and have seen no reason for modifying this conclusion up to date.

Par. 86 Broadly speaking, it is unwise to draw conclusions and make formulae from experiments in which more than one variable is allowed to vary in the same trial. This criticism is made in no sense to belittle the value of the work done by others, but with the object to pointedly call the attention of future experimenters to such errors as have been made primarily by ourselves and also by others.

15 It is only after considerable hesitation that I have decided to answer somewhat in detail the criticism presented upon our paper by Messrs. Adamson and Nicolson, and my principal reasons for doing so will be found in the footnote to Par. 112 of "On the Art of Cutting Metals." In considering the criticism of these gentlemen, it should be understood that advance proof sheets, printed upon very poor paper, were sent to them, and that when they wrote the greater part of their criticism only about two-thirds of the reading matter of the paper was in their possession, and probably not one-third of the tables and cuts which were finally printed on the folders; and also that only a very short time was given them in which to read the paper and write their critiques. Much of what has been said by these gentlemen must, therefore, be attributed to a hasty reading of a very imperfect paper.

16 For example, both criticise the title of the paper as being entirely too broad. On the outside of the Proceedings and at the opening chapter of the paper, the title is "*On* the Art of Cutting Metals." In hastily reading this title, however, both Messrs. Nicolson and Adamson have overlooked the word "on." Certainly the assumption that this paper dealt with even any large part of the art of cutting metals would have been false. It is, however, proper, I think, to say that the paper is written "*On* the Art of Cutting Metals."

17 Again, in Par. 13 of Mr. Adamson's discussion, I quote as follows:

"There are many conclusions given in Mr. Taylor's paper as the result of his long continued experiments, but the grounds upon which they are based, or the method by which they may be applied in practice, are omitted. For example, I may refer particularly to Par. 279 to 288, etc."

18 If we had (as Mr. Adamson says) given the conclusions stated in Par. 279 to 288 without further reference to the data upon which these conclusions are based, Mr. Adamson's words of censure would have been entirely inadequate. The conclusions from Par. 279 to 288, in fact, constitute a very brief summary of almost all of our experiments, and a little more careful reading on the part of Mr. Adamson would have shown him that for each of these conclusions there is an entire chapter in the paper, giving in detail both the experiments and the reasoning upon which the conclusion is founded.

19 It is true that in the Manchester Report every important detail regarding the individual cut of each tool experimented with is recorded, and this data is both interesting and valuable. The work of the Manchester Committee, however, includes, all told, only about 220 experiments, and in describing these experiments, including the discussion, 130 pages of printed matter were issued by Messrs. Adamson and Nicolson. It will be remembered, on the other hand, that we made and recorded between thirty and fifty thousand experiments. If we, therefore, had taken proportionately the same amount of space in recording and describing and in the discussion of our experiments as was taken by Messrs. Nicolson and Adamson in describing the Manchester experiments, our work would have filled a volume of say about 24,000 pages. It is with regret that I read in Par. 29 of Mr. Adamson that he was "nauseated" by the bulk of our paper and our constant repetition of facts. He would indeed, however, have been a sick man if we had followed his advice and recorded each one of our forty thousand experiments, as was done in the work of the Manchester Committee.

20 We must also attribute to a hasty reading of our paper Mr. Adamson's remarks in his Par. 21, in which, if I read him aright, he rather insinuates that we are holding back some much needed data. In the Manchester experiments, steel forgings were classified only into three broad classes, "soft," "medium" and "hard," and he accuses us of withholding in our paper data and constants relating to all except "medium steel." Mr. Adamson has, however, failed to note on our slide rules (Folder 11, Fig. 79) that we divide the metals worked upon in a machine shop into 40 classes, instead of only three. He has overlooked our Par. 1133, etc., regarding the classification of

metals and also our large table (Folder 23, Table 141), in which we record the chemical and physical properties of 40 different forgings, with their appropriate cutting speeds and a formula which expresses fairly well the relation existing between cutting speed and tensile strength, and percentage of elongation in steel forgings.

21 Answering Mr. Adamson's criticism in Par. 21, that our formulae are too elaborate: Our formulae are all intended for use in making slide rules. These slide rules must be made by something of a mathematician, though there is but little difficulty in constructing them, once the necessary formulae are given. In our judgment, no shop foreman should be bothered to use a formula, however simple. The slide rules can be used by any mechanic, and require no mathematical knowledge whatever for their use.

22 In the reference, in our paper "On the Art of Cutting Metals," to the Manchester experiments conducted by Messrs. Adamson and Nicolson, etc., our principal criticism was that these experiments while they gave exceedingly interesting and valuable data—the most complete heretofore published—yet they were not conducted upon truly scientific principles, mainly for the reason that the joint effect of four or five variables was experimented upon at the same time. Our main contention throughout our paper is for the necessity of determining the effect of each one of the twelve variables independently. A considerable portion of both Mr. Nicolson's and Mr. Adamson's criticism consists in defending the method used in the Manchester experiments of determining the joint effect of several variables as being correct. With this part of their criticism I must take issue, directly and emphatically. It is by no means correct for Mr. Nicolson to state, as he does in Par. 13 to 23, and particularly in Par. 15, that his formula, which omits eight of the variables in the problem, is to be depended upon. These eight variables, which Mr. Nicolson implies do not exist, and which his formula in Par. 13 entirely ignores, are facts, and facts which have taken us years of patient and costly work to investigate. It would be useless for us to again point out the existence and the effect of each of these eight variables upon the problem. As a single sample, however, of the eight, I would refer to the thickness of the shaving which we, in our paper, treat as one of the most important of the variables calling for investigation. Mr. Nicolson, in Par. 10 to 13, etc., attempts to prove that the thickness of the shaving is not an independent variable. His claim is, in effect, that doubling the thickness of the shaving would have the same effect on the cutting speed as doubling the depth of the cut, for example; or, in other words, that the real variable is the sectional area of the shav-

ing, namely, the thickness of the shaving multiplied by the depth of the cut. He has evidently completely overlooked or misunderstood the experiments described in Par. 292 to 298, and in Par. 299 to 306, in which it is most conclusively proved that a given variation in the thickness of the chip produces three times as much effect upon the cutting speed as a corresponding variation in the depth of the cut produces.

23 Mr. Nicolson has also evidently overlooked all that we have written in the paper as to the effect which the curve of the cutting edge of the tool has, first, upon the thickness of the shaving, and indirectly on this account upon the cutting speed of the tool. Mr. Nicolson in his criticism implies, if he does not directly claim, that the curve of the cutting edge of the tool has practically no effect upon the cutting speed, as this is one of the eight variables of which his formula takes no account, and which he claims need not be considered. And yet it is a fact, as stated by us in conclusion, Par. 282, and in greater detail in the chapter on "The Line or Curve of the Cutting Edge," beginning with Par. 1169, that a tool having a broad cutting edge (the effect of which is to diminish the thickness of the shaving to such an extent that it becomes exceedingly thin), can be run at six times as high cutting speed as, for instance, a thread tool can be run, or an old-fashioned diamond point tool. Surely a formula such as Mr. Nicolson's should not be used which neglects a variable so important as this. We have gone to great length in the paper to make it clear that it is the thickness of the chip which is the main factor, in allowing high cutting speeds for tools with broad cutting edges. And yet Mr. Nicolson claims that neither the thickness of the chip nor the shape of the cutting edge of the tool need be practically considered in the problem.

24 It is entirely true that a tool could be made, having such a curve for its cutting edge that, for example, a cut with a depth of $\frac{1}{8}$ of an inch and a feed of $\frac{1}{8}$ inch would have almost the same cutting speed as would accompany a cut of $\frac{1}{2}$ inch depth and $\frac{1}{32}$ inch feed. But this result would be brought about because the curve of the cutting edge had been carefully so shaped that the thickness of these two shavings would be practically the same. It is likely that this possibility has been overlooked by Messrs. Nicolson and Adamson. I would, therefore, refer them to diagrams on Folder 16, Fig. 111 and Fig. 112,and to diagrams on Folder 17, Figs. 116 and 120, with the reading matter in the body of the text which accompanies these diagrams. Comparatively little study of the principles there discussed will enable anyone to make a tool having a cutting edge in which the thickness of the shav-

ing will be practically the same for these two cuts, which differ so greatly in their depth and in the coarseness of their feed, and it is even possible that, having accidentally used a tool approximating to this shape in their experiments, may have misled Messrs. Nicolson and Adamson into the false belief that the thickness of the shaving need not be considered as an independent variable.

25 In walking through a machine shop, it is the larger and heavier cuts which attract most attention, and are of the greatest interest. And we are prone to forget that machinists, who are using the smaller and less spectacular lathes and machine tools, are paid *about* the same wages as those who are running the larger tools, and that, therefore, it is just as important that proper combinations of cutting speed and feed should be used on the small machines as on the large.

26 It is not unnatural therefore that Mr. Nicolson should fall into the error, as he does in Par. 16 and following, of implying that in experimenting upon the laws of cutting metals, feeds finer than $\frac{1}{16}$ of an inch are of comparatively little consequence. (In the Manchester experiments no feeds finer than $\frac{1}{16}$ inch were used.) It is a fact, however, that in the average machine shop throughout this country, more cuts are taken by tools with a feed as fine as $\frac{1}{32}$ inch than are taken with coarser feeds than this. Therefore, it becomes of the greatest importance to investigate the laws affecting feeds as fine as and finer than $\frac{1}{32}$ inch, and we have devoted a very considerable portion of our paper to describing experiments of this nature. Mr. Nicolson, in inferring, as he does in his Par. 16, that we mean in our statement in Par. 252 that "experiments made on cuts with a feed as light as $\frac{1}{32}$ of an inch are worthless for determining the laws of cutting speeds, has failed to understand the meaning which we intended to convey in this paragraph, namely, that if one intended to investigate such elements as the following for example, the quality of a given tool, or the effect of a definite type of treatment for a tool, or the effect of a stream of water in cooling a tool, it was inadvisable to use a feed smaller than $\frac{1}{16}$ inch or a depth of cut less than $\frac{3}{16}$ inch. In experiments made for such purposes this is true, because of the increased difficulty of maintaining uniform conditions when feeds finer than $\frac{1}{16}$ inch or a depth of cut more shallow than $\frac{3}{16}$ inch are used.

27 It is, however, equally true that any series of experiments for determining the laws of cutting metals or any general formulae for practical use are exceedingly incomplete which fail to deal with feeds finer than $\frac{1}{16}$ of an inch.

REPLY TO MESSRS. LE CHATELIER AND CARPENTER

28 Mr. Le Chatelier, through his invention of the electro-thermal pyrometer, and through his many investigations made possible through this invention, as to the chemical changes which take place in steel as evidenced by the "points of recalescence," or "critical points" in the heating and cooling of steels, together with their corresponding microscopic structures, has properly earned the title of the father of this branch of metallurgical science; and Mr. Carpenter, through his recent improvements and thorough and careful work, has added greatly to our knowledge, especially as to the facts and theories connected with modern high speed tools. We therefore feel especially complimented that these gentlemen should have taken the trouble to criticise our paper, and I wish to assure them that their kindness is most highly appreciated.

29 Referring in detail to the criticism of Messrs. Le Chatelier and Carpenter, Mr. Carpenter, in Par. 3-a, indicates that "martensite" is the proper name for "hardening carbon," and not "cementite." I have no doubt that Mr. Carpenter is correct.

30 I am also prepared to accept Mr. Carpenter's larger experience in stating, in Par. 3-b, that hardening carbon changes to softening carbon at between 700 to 720 degrees C.

31 That element of Mr. Carpenter's criticism, however, which has the greatest interest for the writer, is his endeavor to coördinate "hardness" and "red hardness," which he does in Par. 5 to 11. Mr. Carpenter's reasoning on this subject is briefly answered by Mr. Le Chatelier, in Par. 14 of his discussion.

32 It seems desirable, however, for me to supplement Mr. Le Chatelier's remarks (made without any knowledge of Mr. Carpenter's criticism) by the following: My understanding of Mr. Carpenter's theory is that it is his opinion that the highest degree of red hardness is attained at the temperature at which the austinitic structure is changed to the martensitic, in the case of high speed steels. Now, if the martensitic structure represents steel of this type in its hardest condition—and I understand this to be the case—then I can state from extended observation that the highest degree of red hardness is not necessarily accompanied by the martensitic structure; because Mr. White and the writer examined quite a number of tools of approximately the same composition, namely, Tool No. 27, Folder 20, and all of which showed the highest degree of red hardness which had been obtained by us at that time; and these several tools, in spite of having the same degree of red hardness, each showed a different microscopic

structure; and, what seems of equal importance, they showed a very great variation in their hardness as tested with the file; some of them being exceedingly soft, while others were very hard. As stated before in our paper, we have obtained the highest degree of red hardness by burying tools in lime immediately after heating, say, to 1200 degrees C.; and also in a tool of similar composition by quenching in cold water after heating to 1200 degrees C. In the case of the lime-cooled tool, as you may imagine, it was very soft when tested with the file, while the water-cooled tool was very hard, and yet the same degree of red hardness was found in the two tools—*i. e.*, they both had the same "standard speed." We have never, however, made a series of experiments of this kind with tools of the composition of No. 1 on Folder 20, and it may be that the highest degree of red hardness could only be attained in the case of steel of No. 1 composition when it has the martensitic structure. All of our experience, however, would indicate the probability that with steel of this composition the highest degree of red hardness could be attained with several different microscopic structures; and if this proved to be true, as we found it true in the original high speed steels with which we experimented, then I feel that we would be justified in repeating our assertion that there is no traceable relation between hardness and red hardness.

33 It would be exceedingly interesting to make experiments of this kind with steel of No. 1 composition, Folder 20. No doubt, a lime-cooled tool would exhibit a totally different microscopic structure from a water-cooled tool, and I believe that in cutting medium or soft metals the lime-cooled tool would show practically the same red hardness that would be shown by the water-cooled tool. I think that the description given by Mr. Carpenter in his discussion (Par. 7 to 10) of the relation of microscopic structures to the elements of tungsten chromium on the one hand and to red hardness on the other hand, is the simplest and clearest piece of writing on this subject that has come to my attention. I am very much afraid, however, that it does not represent the facts with regard to red hardness, which, after all, is the useful property of high speed steels.

34 I hope that Messrs. Le Chatelier and Carpenter will continue their most interesting investigations, and it will give me great pleasure to coöperate with them at any time.

35 It is needless to repeat, however, that a thorough test for red hardness in tools, according to the method of "heating and running," is both expensive and takes a great deal of time and calls for elaborate apparatus, and that nothing short of a very careful and thorough test of this nature should be undertaken.

REPLY TO MESSRS. J. E. STEAD AND JOHN KEY

36 Messrs. J. E. Stead and John Key, in Par. 2, speak of the trouble which comes from wear of the lathe center in turning with high speed tools. This can be obviated by providing the lathe with centers made of a high speed steel, properly treated. The quality of red hardness in these centers is just as valuable for resisting wear at high rotative speed of the work in a lathe as the same quality is in high speed tools for cutting the metal.

REPLY TO MR. BLAUVELT

37 I have not yet had the opportunity to read the patent referred to by Mr. Blauvelt. Mr. Blauvelt justly says, however, that it is not a matter of surprise that similar investigations should be carried on by two different sets of people entirely without knowledge on the part of either. This is a matter of very frequent occurrence.

REPLY TO MR. W. S. HUSON

38 I have never found a single instance in which it was impossible to do accurate roughing work with high speed tools, and I feel sure that if Mr. Huson had himself experimented on a series of printing press beds, such as he refers to, that he would have succeeded in getting as true work with high speed tools as he did formerly with the old low speed. There was something wrong, either with the shape or treatment of his tools, or with his method of setting the work on the planer. We find it, however, almost uniformly the case that a machinist who has worked for perhaps years on a particular class of work at a given speed, with the old grade of tools, will maintain that this particular kind of work cannot be machined with the high speed tools, and someone must come in from the outside and prove that the work can be just as accurately done with the high speed tools as with the old style.

REPLY TO MR. FRED. M. OSBORN

39 I must take exception to Mr. Osborn's statement that the original Mushet tools can properly be called high speed tools. The analyses of the Mushet tools, one made in 1898 and the other in 1894, will be found in Nos. 65 and 71 of our Folder 22, Table 140. It will be seen that one of these tools contained 0.398 of chromium, while the other contained 0.490 of chromium, and that both of these tools contained more than 2 per cent of manganese. It is our observation that no tool can be called in any sense a high speed tool which does not

contain more than 0.50 per cent of chromium. During the many years in which Mushet tools were used in this country, I met many times the salesmen who were introducing Mushet self-hardening steel, and I also had the pleasure of meeting Mr. Jones, as referred to by Mr. Osborn, and not one of these men ever suggested that better results could be obtained with Mushet tools by heating them to a high heat and then blowing them in a blast of air. On the contrary, their directions were most positive not to heat the Mushet steel beyond a cherry red for obtaining the best results, and as an indication of the proper heat a red colored label was pasted on to much, if not all, of the Mushet steel.

40 It was the substitution of chromium for manganese in self-hardening steel which paved the way for the modern high speed steels, and this substitution was not made in the Mushet steel by Messrs. Osborn & Company. It was made, however, by a number of makers of self-hardening steel who were competing with the Mushet steel.

REPLY TO MESSRS. RICE, BANCROFT, HATHAWAY AND LEWIS

41 The criticisms of Messrs. Rice, Bancroft, Hathaway and Lewis are of the greatest interest both to me and my associates, as dealing with the broader practical application of the facts recorded in the paper. Our interest in the scientific, or what one might call the intellectual element of the art of cutting metals, has been of course very great; but after all the true measure of the value of this work should be the actual every-day help which will be given by these laws to the superintendents, foremen, and mechanics in our machine shops. And the large practical results to be realized from this work are, in my opinion, to such an extent bound up with the general principals of our whole system of management, that it is the relation of the laws which we have determined to the mechanism which we have developed for their application to this system of management, which holds for us the greatest interest.

42 I must again repeat that I feel quite sure that the real benefit will only be realized from these laws when they are used for the purpose for which the investigation was originally started, and for which it was carried out through so many years, namely, that of enabling the management of a shop to assign a daily task in advance to each machinist, giving him written instructions as to the shape of tool, the depth of cut, the feed, the speed which he must use, and the total time which he is to take in doing each job, and then paying him a large bonus for carrying out his orders. When the human and other mechanism is once established in a shop for accomplishing this end,

results which are truly astounding are sure to follow, and we feel especially indebted to Messrs. Bancroft, Hathaway and Lewis for indicating this fact. And I wish again to emphasize what I have before said, that anxious as we are to have these laws of cutting metals and our slide rules properly used, and more particularly to have the general principals of our system of management adopted, we hesitate greatly in advising any one to attempt a change from the old methods to task management, without the regular assistance of men who have been especially trained in doing this work; and Mr. Lewis has rendered a service to those intending to adopt these principles by so clearly calling attention to this fact.

FINAL CLOSING REMARKS

43 I wish to say for myself, as well as on behalf of Messrs, White, Barth and Gantt, that we have been fairly overwhelmed with the commendation which our paper has received from so many men, and also from the technical journals, both in this country and in Europe. We are especially pleased and grateful that men of such eminence and of such widely divergent interests as Messrs. Henry R. Towne, James M. Dodge, Calvin W. Rice, J. S. Bancroft, H. K. Hathaway, Wilfred Lewis, J. T. Hawkins, L. P. Breckinridge, Oberlin Smith, W. S. Huson, and W. H. Blauvelt, in this country, should have taken both the time and trouble to read and to criticise our work from their various viewpoints. And it is in some respects even more gratifying that we should have had the added help and criticism of such noted foreigners as Monsieur H. Le Chatelier, the most eminent French metallurgist, and Monsieur C. Codron, the able experimenter and analyst, who has written upon many of the elements of the art of cutting metals, as well as that of the several English authorities who are perhaps best qualified to criticise this work, namely, Mr. H. C. H. Carpenter, Dr. J. T. Nicolson, Mr. Daniel Adamson, Mr. S. Skeggs, Mr. J. E. Stead, Mr. John Key and Mr. F. M. Osborn.

44 It is our hope that our paper may excite a sufficiently lasting interest in the subject to induce others to continue the much needed experiments in this field, and it is needless to add that we shall be at all times not only ready but most pleased to coöperate with any other experimenters.

45 In conclusion, I can but express a certain surprise and regret that our paper "On the Art of Cutting Metals" has attracted so much more attention than has been given to our various papers on shop management, which, after all, is the real vital subject in which we are most interested, and of which the art of cutting metals constitutes, on the whole, merely one of the important elements.